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ART. XXI.—*On the comparative Expenditure of Heat in different forms of the Air-Engine;* by FREDERICK A. P. BARNARD, Professor of Chemistry and Natural History in the University of Alabama.

IN the year 1840, an air engine of a very simple construction was patented in England by the Rev. Robert Stirling. The effective force in this engine was derived from the fluctuations of pressure created by the sudden alternate heating and cooling of a body of confined air, which was driven from end to end of the containing vessel by the motion of a solid plunger occupying about three quarters of the entire cavity. In its passage, it passed through a regenerator constructed of parallel laminæ of copper; and while one end of the air-vessel was exposed to heat, the other was furnished with a refrigerating apparatus. To the cold end was attached the working cylinder. This engine was successfully operated for several years, at the Dundee iron foundry, and perhaps elsewhere. It was found to be largely more economical than a steam engine, of corresponding power. No advantage, it will be perceived, was taken of compression pumps, to give to the air a higher pressure than that of the reservoir; and, on the other hand, the engine had not the resistance to contend with, which such pumps occasion. The confined air, however, was originally compressed to a great density. In the smaller engines a maximum pressure of 360 lbs. to the square inch was carried—equal to 24 atmospheres—but in the larger ones the pressure rose no

higher than to 240. The maximum temperature usually employed was 650° F.

In examining, in the March No. of this Journal, Mr. Joule's project of an air engine, I assumed, what I think can easily be proved, that the plan there proposed is the most economical of all which have been suggested, and which are at the same time practicable. In Ericsson's, the compression cylinders of Joule, and the regenerators of Stirling, have been united; but I have shown that the regenerators are not essential to economy. It has however, become a matter of interest to compare with each other the several forms which it has been proposed to give to the air engine, in regard to their economical merits; and it is the object of this paper to present the means for making such a comparison.

Ten years ago, the problem here under examination would hardly have admitted of an *a priori* solution. The theoretical and experimental researches of Rankine, Joule, Thompson, Regnault, Clausius, Meyer, and others, have within a recent period, put it within the reach of an easy calculation. I shall assume that it has been established by these authorities:

1st. That when a measurable mechanical effect is produced by heat, a corresponding definite amount of heat ceases any longer to exist as such; and that the measure of this amount has been satifactorily ascertained.* This proposition is convertible.

2d. That when heat is absorbed by a solid or a liquid, it is in part expended in the internal mechanical effect of overcoming cohesion, and in part in the external effect of pressure upon surrounding resistances; while in part it remains sensible: but that, in the case of a perfect gas, there is no internal expenditure; so that all the heat absorbed may be strictly accounted for either in outward work, or in elevation of temperature, or in both.

3d. That consequently, when a gas expands against pressure, but is maintained at the same constant temperature by a just supply of heat, the entire amount of heat so supplied is converted into outward mechanical effect: but that when, during expansion, the temperature rises, more heat is supplied than is so converted; and when it falls, less.

4th. That though no gas is perhaps rigidly perfect, yet atmospheric air, and those aëriform bodies generally, which are not reducible by pressure and cold to the liquid state, are sensibly so.†

5th. That equal volumes of different gases, however different in density or specific heat, when taken at the same pressure and

* Mr. Joule's severe investigation of this point has fixed the "mechanical equivalent" of a "unit of heat"—that is, of the quantity of heat which would raise the temperature of a pound of water one degree F.—at 772 pounds lifted one foot.

† The properties of a perfect gas are expressed in the law of Mariotte, viz: that the pressure is inversely as the volume, temperature being constant; and in that of Gay Lussac, viz: that the pressure is directly as the temperature (above the absolute zero), volume being constant.

temperature and equally expanded by heat under that constant pressure, perform equal quantities of work, and therefore convert into mechanical effect equal quantities of heat.

6th. That equal volumes of different gases, taken at the same pressure and temperature, and maintained during expansion at the same unvarying temperature, convert into mechanical effect different quantities of heat, proportioned to their specific heat estimated according to volume.

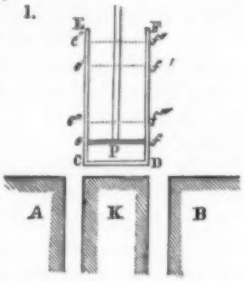
7th. That different gases expanding against pressure, without any accession of heat from without, convert into mechanical effect a portion of the heat which they previously held sensibly, the temperature at the same time falling; and that this depression of temperature is the more rapid in proportion as the specific heat of the gas is less. This proposition and the last are convertible.

These propositions, with the aid of the specific heats of the gases furnished by Regnault, and of the formulæ of Poisson for the effect upon pressure and temperature produced by change of volume without any transfer of heat to or from the gas undergoing change, furnish all the data necessary for computing the power of any species of engine driven by the elastic force of heated gas.

It may be observed, in the first place, that the differential pressure which constitutes the driving force of an air engine may be obtained in either of two ways: first, by employing a condensing apparatus, worked by the engine itself, to compress a given mass of air, and then to sustain for a time the pressure thus secured, by application of heat; or, secondly, to subject a similar mass of air, confined in a tight vessel, simply to large alternations of temperature, without employing any auxiliary contrivance for compression. In some projects, a resort to both these expedients is aimed at; but in general one or the other will give a distinctive character to the engine.

Some projected air-engines propose to draw a fresh supply from the atmosphere at every stroke, and to discharge it when the stroke is completed; while others are designed to employ the same mass of air over and over again. In discussions of the relative advantages possessed by these different forms of construction, this difference is unimportant. We may suppose, in every case, that the mass of air is constantly unchanged; for in those in which a discharge takes place, we may suppose the discharge to be directly from the working to the supply cylinder, provided we suppose the air to be so effectually refrigerated on its way, that it shall encounter no greater resistance than that which the atmosphere itself would oppose to its free discharge. This being premised, we shall simplify the theory of the air engine, by supposing that all the fluctuations of temperature and pressure which the air undergoes, take place within the working cylinder itself. The fol-

lowing illustration is from Carnot, by whom the first attempt was made, in 1824, to present a dynamic theory of heat.*



Let A be a body constantly at the temperature S°, and capable of preserving another body in contact with it at the same temperature. Let B be a second body, at the lower constant temperature T°, and capable, in like manner, of preserving another body in contact with it at the same temperature. Let CDEF be a cylinder in which moves an air-tight piston P; both cylinder and piston being impervious to heat, with the exception that the bottom, C D, of the cylinder is a perfect conductor, and without capacity for heat. K represents a stand, impervious to heat, designed, when necessary, to neutralize the conducting power of C D.

If this cylinder contain beneath the piston P, in its present position, a gas, or liquid capable of conversion into vapor, and be placed upon the body A, the piston is to be supposed to rise, while the confined gas, or liquid and vapor, is kept at the temperature S°, by the body A. If now we force down the piston to its original position, the body A being supposed to withdraw heat as it before imparted it, and always to maintain the temperature of the gas at S°, we shall have to expend precisely as much force as was developed during the expansion. But if, when the piston has reached the top of the cylinder, we remove the whole to the body B, and suppose the temperature to be instantly reduced by this body to T°, and afterward maintained at that point, then we may force down the piston with less labor than before. Thus the rising of a piston in a cylinder standing on A may force down one in another equal cylinder on B, and preserve an excess of power to be applied to other purposes.

It is practically difficult, if not impossible, by mere refrigerating contrivances, to reduce the temperature of a body of gas or vapor within such a cylinder, with sufficient rapidity or sufficient economy, to render eligible that mode of obtaining a reduction of pressure. But if we suppose the cylinder to be removed from A when the piston has reached a position short of the top, as $e'f'$, and placed on K to prevent any further supply or withdrawal of heat, and then suppose the piston to rise by the elastic force of the gas or vapor beneath, the temperature will fall by the conversion of sensible heat into mechanical effect, and artificial refrigeration will be unnecessary. Suppose that, when the piston reaches $e''f''$, the temperature is the same as that of B. The

* See Thompson's "Account of Carnot's Theory," in the Trans. of the Roy. Soc. of Edinburgh, vol. xvi, part 5, 1849.

cylinder may then be transferred to that body, and the piston pressed down, the temperature remaining at T°, until a third position, $e'''f'''$, is reached, which is such that, on again placing the cylinder on K, and completing the downward stroke, the temperature will again rise to S°. It thus appears that, of a certain definite amount of heat drawn from A, a certain less amount is given up to B, and the difference is converted into mechanical effect.*

It is evident that, by reversing this whole process, that is, by first placing the cylinder on K, and allowing the piston to rise to $e'''f'''$, thereby depressing the temperature from S° to T°, then placing the cylinder on B, and allowing a further expansion to $e''f''$, the temperature being kept at T° by heat received from B, then transferring the cylinder to K again, and forcing down the piston to $e'f'$, so as to raise the temperature to S°, we may finally, by completing the downward stroke in contact with A, transfer to that body not only as much heat as was imparted by B, but also the additional amount expended in compression, above what was received during the expansion.

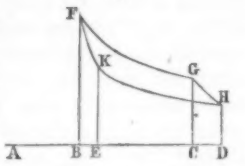
A gas or vapor engine constructed in conformity with this principle, is capable, therefore, if worked backward by force, of generating as much heat as is expended in its direct action, and of restoring the heat so generated to the original source. Such an engine is what Carnot defines to be a *perfect* thermo-dynamic engine. It is obvious from the nature of things, that no other description of engine, moved by heat, can convert a larger fraction of the heat drawn from the source into available power, than one of this kind: for, if there could be such an one, it would be capable of driving this one backward, and of thus constantly refunding to the source as much heat as it withdraws, while still preserving a balance of positive power—or, in other words, a balance of effect without a cause—which is impossible. We are thus furnished with a natural limit to the power obtainable by the expansion and compression of elastic fluids between given limits of temperature. Our first object then, must be to find a general expression for this effect, for the perfect engine. The illustrations which immediately follow are substantially derived from Thompson and Clausius, and would be superfluous, had the investigations of those writers been republished in this country.†

* According to Carnot's theory, which was identical with that which has hitherto been universally received, the amount of heat given up to B was *not* less than that derived from A, but exactly equal to it: hence the mechanical force exerted was presumed to be a natural concomitant of the transfer of heat from a hotter to a colder body, and not a conversion of the heat, or of any part of it, into mechanical effect.

† Clausius, on the Moving Force of Heat, &c., from Poggendorff's Annalen, vol. lxxix, republished in the Lond. and Ed. Phil. Mag., July and August, 1851. Thompson, in the paper above quoted—also Lond. Phil. Trans., part 1, 1852, and elsewhere.

Suppose a column of atmospheric air, whose altitude is equal to A B, to be confined in a cylinder by a piston capable of moving without friction. Let the pressure it exerts upon the piston at the temperature S° be represented by the ordinate B F. Let it expand with unvarying temperature until the piston reaches the point C, and let the ordinate C G represent the pressure at that time. Let it now expand without receiving or imparting heat, from C to D, during which time the temperature falls to T°, and the pressure to D H. Let it now undergo compression, maintaining the invariable temperature T°, until the piston reaches E, a point such that the further compression, (without gain or loss of heat,) to the original volume, A B, shall restore the original temperature S°, and pressure, B F. It is evident that the area, F G H D B will represent the entire mechanical effect produced during the expansion, and the area, F K H D B will represent the force required to restore the air to its original state. Hence, the differential area, F G H K, will be the measure of the amount of heat converted into available force.*

2.



Represent the original volume of the air, A B, by V', and the volume A C by V''; also A D by $V_{,}$ and A E by $V_{,,}$. Put τ' for the original temperature, S°, reckoned from the absolute zero (taken at 459° below 0° F.), and $\tau_{,}$ for the temperature T°, similarly reckoned. Then, according to Poisson's formula for temperatures as affected by expansion or compression,

$$\frac{\tau_{,}}{\tau'}=\left(\frac{V''}{V_{,}}\right)^{\gamma-1}, \text{ and also } \frac{\tau_{,}}{\tau'}=\left(\frac{V'}{V_{,,}}\right)^{\gamma-1}; \text{ or } \frac{V''}{V_{,}}=\frac{V'}{V_{,,}}$$

Now the area, G H D C, which measures the mechanical effect produced during the second expansion, may be found by integrating the expression $\int pdv$ between the limits V'' and $V_{,}$; substituting for p its value from Poisson's formula for pressure under these circumstances, viz.:

$$p=P''\left(\frac{V''}{v}\right)^{\gamma},$$

where P'' represents the pressure corresponding to V''. Hence the area in question is expressed by the formula,

$$\frac{P''V''}{\gamma-1}\left(1-\left(\frac{V''}{V_{,}}\right)^{\gamma-1}\right).$$

And, in like manner, P' representing the pressure corresponding to V', the area F K E B, which measures the force expended in the final compression, is expressed by

* Since the surrounding medium—the atmosphere, for instance—aids the compression as much as it opposes the expansion, no account need be taken of it.

$$\frac{P'V'}{\gamma-1}\left(1-\left(\frac{V'}{V_{''}}\right)^{\gamma-1}\right).$$

But, according to Mariotte's law, $P'V'=P''V''$; and we have just seen that $\frac{V''}{V_{'}}=\frac{V'}{V_{''}}$: whence the area G C D H is equal to the area F K E B; or, in other words, the second expansion is exactly balanced by the second compression:—a conclusion which we might, indeed, have independently drawn, since the same amount of heat which disappears in the expansion, reappears in the compression.

The area F G C B, according to the third general principle stated above, is the measure of the total amount of heat which the air has received from the source, converted into mechanical effect. To find its value we observe, first, that if p, v and t represent the simultaneous pressure, volume and temperature (reckoned from the absolute zero) of any gas, the expression $\frac{pv}{t}$ will be, for that gas, a constant quantity. And if we take p_0, v_0 and t_0 to express the values of these variables under ascertained circumstances, as for instance at Fahrenheit's zero, and under a given barometric pressure, then $\frac{p_0 v_0}{t_0}$ may be put $=R$, and in any other condition of the gas we have $pv=Rt$, and $p=\frac{Rt}{v}$.

Now the differential of F G C B is pdv; whence, representing it by M (mechanical effect), we have,

$$M=\int\frac{Rt}{v}dv=\int\frac{R\tau'}{v}dv=R\tau'\text{ h. l. }\frac{V''}{V'},$$

between the limits V' and V''.

In like manner, area K H D E ($=M'$) will be

$$M'=R\tau_{'}\text{ h. l. }\frac{V_{'}}{V_{''}}=R\tau_{'}\text{ h. l. }\frac{V''}{V'}.$$

And the differential area, F G H K, which we will represent by W, will be

$$W=M-M'=R(\tau'-\tau_{'})\text{ h. l. }\frac{V''}{V'}.$$

If H, then, be the total amount of heat received by the gas, and of which the entire mechanical effect is represented by M, the fraction converted into available work will be found thus:

$$M:W::R\tau'\text{ h. l. }\frac{V''}{V'}:R(\tau'-\tau_{'})\text{ h. l. }\frac{V''}{V'}::H:H\frac{\tau'-\tau_{'}}{\tau'}.$$

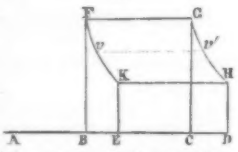
It is geometrically evident that no larger a fraction of the heat absorbed by a gas fluctuating between the temperatures τ' and $\tau_{'}$ can be made available, than that here represented; for the differ-

ential area cannot be enlarged unless the curve F G rise in some point above, or the curve K H descend in some point below, the logarithmic curve; which neither can do without transcending the limit v' or $v_{,}$.

The practical difficulty of maintaining a body of air, while undergoing dilatation or compression, at an unvarying temperature, will render it always, probably, an impossibility to work an engine on this principle, and therefore to realize so large an advantage from the heat expended, as the extremes of temperature in furnace and refrigerator might lead us theoretically to anticipate. It is easy, however, to secure a constant *pressure*, and this presents the question of economy under a new form.

Suppose the air to expand from B to C, with rising temperature, and a constant pressure represented by F B = G C, then to expand, without receiving or imparting heat, to D, at the lower pressure H D, then to be compressed to E, with loss of heat and constant pressure E K = H D, and finally to be compressed to its original bulk, acquiring at the same time by the compression, and without receiving or imparting heat, the original temperature and pressure. The differential area, F G H K, bounded by the parallel straight lines F G, and H K, will represent the balance of positive mechanical effect. If we draw a line, as $v\,v'$, parallel to F G, it will represent the difference of volume of the air when at equal pressure in the two opposite processes of expansion and compression. Calling these volumes v and v', and the corresponding pressure p, we shall have (using the other symbols as before),

3.



$$p = P'\left(\frac{V'}{v}\right)^{\gamma} = P''\left(\frac{V''}{v'}\right)^{\gamma}$$

Or, as $P' = P''$, $\frac{V'}{v} = \frac{V''}{v'}$, and

$$V'' : V'' - V' :: v' : v' - v = \frac{v'(V'' - V')}{V''}$$

But $v' = V''\left(\frac{P''}{p}\right)^{\frac{1}{\gamma}}$; whence $v' - v = (V'' - V')\left(\frac{P''}{p}\right)^{\frac{1}{\gamma}}$.

Now area F G H K $= \int_{P_{,}}^{P''} (v' - v)\,dp = \int_{P_{,}}^{P''} (V'' - V')\left(\frac{P''}{p}\right)^{\frac{1}{\gamma}} dp$

Or, observing that dp is negative,

$$W = \frac{\gamma}{\gamma - 1}(V'' - V')\left(P'' - P''^{\frac{1}{\gamma}} P_{,}^{\frac{\gamma-1}{\gamma}}\right) = \frac{\gamma P_{,} V'}{\gamma - 1}\left(\frac{V''}{V'} - 1\right)\left(1 - \left(\frac{V''}{V_{,}}\right)^{\gamma-1}\right)$$

Where $P_{,}$ represents the pressure at D or E, and is found in terms of P'' by the formula $\frac{P_{,}}{P''} = \left(\frac{V''}{V_{,}}\right)^{\gamma}$; after which P' is put for its equal P''.

We have now four temperatures. That which we have called τ' must be raised in the same ratio as the volume is increased during the expansion at constant pressure. Put τ'' for the temperature at volume V″, and also $\tau_{,}$ and $\tau_{,,}$ for those corresponding to $V_{,}$ and $V_{,,}$.

The mechanical equivalent of the total amount of heat in the air at volume V′ and pressure P′, may be found thus. In expanding from V′ to any other volume, v, the pressure (p) becomes

$$p = P' \left(\frac{V'}{v}\right)^{\gamma}$$

And the work done, during an infinite expansion, will be

$$\int_{V'}^{\infty} p dv = \int_{V'}^{\infty} P' \left(\frac{V'}{v}\right) dv = \frac{P'V'}{\gamma - 1}.$$

If, then, γ represent the ratio of the specific heat of air at constant pressure to that at constant volume, the mechanical equivalent of the heat absorbed, while the temperature is rising from τ' to τ'' and the pressure is constant, will be

$$\frac{\gamma P'V'}{\gamma - 1}\left(\frac{\tau'' - \tau'}{\tau'}\right) = M.$$

And, in the value of W above, $\frac{V''}{V'} = \frac{\tau''}{\tau'}$, and $\left(\frac{V''}{V_{,}}\right)^{\gamma-1} = \frac{\tau_{,}}{\tau''}$

which, substituted, give us $W = \frac{\gamma P'V'}{\gamma - 1}\left(\frac{\tau'' - \tau'}{\tau'}\right)\left(1 - \frac{\tau_{,}}{\tau''}\right)$

Whence $$\frac{W}{M} H = \frac{\tau'' - \tau_{,}}{\tau''} H;$$

or the fraction of the heat absorbed which is converted into available work, is equal to the range of depression divided by the maximum temperature, as before.

In an air engine which discharges the air from the working cylinder at a pressure above that at which it is received into the supply cylinder, a less simple formula is required to express the economical ratio. In this case, if the valves are adequate to afford instant relief to the excess of pressure, the effect is the same as if the air, without being set free, were suddenly refrigerated to such an extent as to reduce the pressure to an equality with that of the supply before compression.

In this case, F B C G may represent, as before, the work, done at the constant pressure P′, and G C D H, that during the second expansion. D E K L will be the representative of the effect of the first compression; and F B E K that of the second.

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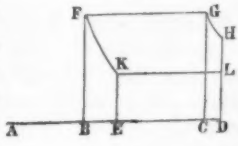


Then $FBCG = P'(V'' - V') = P'V'\left(\frac{V''}{V'} - 1\right) = P'V'\left(\frac{\tau''}{\tau'} - 1\right)$

And $GCDH = \int_{V_{,}}^{V''} P''\left(\frac{V''}{v}\right)^{\gamma} dv = \frac{P''V''}{\gamma - 1}\left(1 - \left(\frac{V''}{V_{,}}\right)^{\gamma-1}\right) = \frac{P'V'}{\gamma - 1}\frac{\tau''}{\tau'}\left(1 - \frac{\tau_{,}}{\tau''}\right)$

Also $KEDL = P_{,,}(V_{,} - V_{,,}) = P_{,,}V_{,,}\left(\frac{V_{,}}{V_{,,}} - 1\right) = P'V'\left(\frac{V'}{V_{,,}}\right)^{\gamma-1}\left(\frac{V_{,}}{V_{,,}} - 1\right)$

And $FBEK = \frac{P'V'}{\gamma - 1}\left(1 - \left(\frac{V'}{V_{,,}}\right)^{\gamma-1}\right) = \frac{P'V'}{\gamma - 1}\left(1 - \frac{\tau_{,,}}{\tau'}\right)$

In the third expression above, we have $\frac{V_{,}}{V_{,,}}$, the value of which we obtain by observing that,

$$V_{,} = V''\left(\frac{\tau''}{\tau_{,}}\right)^{\frac{1}{\gamma-1}}, \text{ and } V_{,,} = V'\left(\frac{\tau'}{\tau_{,,}}\right)^{\frac{1}{\gamma-1}} \therefore \frac{V_{,}}{V_{,,}} = \frac{V''}{V'}\left(\frac{\tau_{,,}\tau''}{\tau_{,}\tau'}\right)^{\frac{1}{\gamma-1}}$$

And $KEDL = P'V'\frac{\tau_{,,}}{\tau'}\left[\frac{\tau''}{\tau'}\left(\frac{\tau_{,,}\tau''}{\tau_{,}\tau'}\right)^{\frac{1}{\gamma-1}} - 1\right]$

Whence the area $FGHLK = P'V'\left[\left(\frac{\tau''}{\tau'} - 1\right) - \frac{\tau_{,,}}{\tau'}\left(\frac{\tau''\left(\frac{\tau_{,,}\tau''}{\tau_{,}\tau'}\right)^{\frac{1}{\gamma-1}}}{\tau'} - \tau'\right)\right]$

$+ \frac{P'V'}{\gamma - 1}\left[\frac{\tau''}{\tau'}\left(1 - \frac{\tau_{,}}{\tau''}\right) - \left(1 - \frac{\tau_{,,}}{\tau'}\right)\right] = W.$

The value of **M** is of the same form as before, since it depends only on V' and V''.

Hence $\frac{W}{M}H = H\left[\frac{\gamma - 1}{\gamma}\left(1 - \frac{\tau_{,,}}{\tau'}\frac{\tau''\left(\frac{\tau_{,,}\tau''}{\tau_{,}\tau'}\right)^{\frac{1}{\gamma-1}} - \tau'}{\tau'' - \tau'}\right) + \frac{1}{\gamma}\left(1 - \frac{\tau_{,} - \tau_{,,}}{\tau'' - \tau'}\right)\right]$

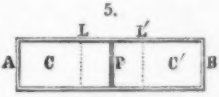
When $\tau_{,,} : \tau' :: \tau_{,} : \tau''$, this expression becomes, as before $= H\frac{\tau'' - \tau_{,}}{\tau''}$

When the maximum and minimum temperatures are given, the economy of working, in an engine of this description, depends on the intermediate temperatures $\tau_{,}$ and τ'. If, when the foregoing proportion holds, we vary $\tau_{,}$ and not τ', we lose, either way. But there may be a slight increase in the economical ratio, by increasing τ' a little above the value $\frac{\tau_{,,}\tau''}{\tau_{,}}$. This fact is illustrated in my article published in the March No. of this Journal. The effect of an increase of τ', is upon the whole to elevate the *source* of the heat, since all that is absorbed is received above this temperature. It is hardly necessary to say that these two quantities may be made to vary, by varying the proportions of the cylinders of the engine, or the position of the cut-off.

In the engine of Stirling, and all others resting on the same principle, we arrive at a similar form of expression for the economical ratio, though the expression for the work done assumes a simpler shape. In order to fix our ideas, we must first examine a theoretic case, which, in practice, it is somewhat difficult to realize.

Let A B be an air-tight cylinder, in which a piston P, capable of moving without friction, separates completely two equal masses of air, C and C′. Suppose that, by the alternate and instantaneous changes of temperature of these masses, the piston traverses the space L L′. When it reaches one of these limits, let the temperature of the air before it be τ', and of that behind it $\tau_{,}$. Then, by an instantaneous change, let the former temperature rise to τ'', the maximum, and the latter fall to $\tau_{,,}$ which is the minimum. There are but two volumes here to be considered, and we may represent the minimum by V and the maximum by V′. Put also P = pressure at minimum volume, and P′ = pressure at maximum volume, both being taken at the end of the stroke, and before the change of temperature. Then the maximum pressure will be $P\frac{\tau''}{\tau'}$; and in accordance with what we have already seen, the positive power exerted at each stroke will be

5.



$$\frac{PV}{\gamma-1}\frac{\tau''}{\tau'}\left(1-\left(\frac{V}{V'}\right)^{\gamma-1}\right)$$

And the resistance $$\frac{PV}{\gamma-1}\left(1-\left(\frac{V}{V'}\right)^{\gamma-1}\right)$$

Whence $$W=\frac{PV}{\gamma-1}\left(\frac{\tau''}{\tau'}-1\right)\left(1-\left(\frac{V}{V'}\right)^{\gamma-1}\right)$$

But, in order to eliminate τ', so that the expression may contain only the maximum and minimum temperatures, which, being those of the source of heat and the refrigerator, may be supposed to be controllable, or known, we may take

$$\tau'=\tau_{,,}\left(\frac{V'}{V}\right)^{\gamma-1} \quad \text{whence}$$

$$W=\frac{PV}{\gamma-1}\left(\frac{\tau''}{\tau_{,,}}\left(\frac{V}{V'}\right)^{\gamma-1}-1\right)\left(1-\left(\frac{V}{V'}\right)^{\gamma-1}\right)$$

Also, as in the former case, the mechanical equivalent of the heat imparted is, (vol. being constant during the heating,)

$$\frac{PV}{\gamma-1}\left(\frac{\tau''}{\tau'}-1\right)=\frac{PV}{\gamma-1}\left(\frac{\tau''}{\tau_{,,}}\left(\frac{V}{V'}\right)^{\gamma-1}-1\right)=M.$$

Whence $$\frac{W}{M}H=H\left(1-\left(\frac{V}{V'}\right)^{\gamma-1}\right)=H\left(1-\frac{\tau_{,}}{\tau''}\right)=H\left(\frac{\tau''-\tau_{,}}{\tau''}\right)$$

If, in the expression for W, above, we make $V_{,}$ variable, we shall find that the maximum effect is obtained from a body of air, of which the minimum bulk and pressure are expressed by PV, when

$$V' = V\left(\frac{2\tau''}{\tau'' + \tau_{,,}}\right)^{\frac{1}{\gamma-1}}.$$

An expansion to a volume approaching or exceeding two-fold is therefore usually necessary to obtain the greatest effect; and the ratio rises rapidly as the maximum temperature is increased while the minimum is constant; the index $\frac{1}{\gamma-1}$ being about 2·44 for air. For a maximum temperature of 480° F., and a minimum of 60°, V′ will be 1·8 V. For 750° maximum and the same minimum, V′ will be 2·25 V.

As the expression $\left(1-\left(\frac{V}{V'}\right)^{\gamma-1}\right)$ is the measure of the heat made available, it is evident that the economy will increase with the expansion; but there occurs here, as in the other form, a negative pressure at the end of the stroke, after passing a certain limit. P′, which has been taken for the final pressure, will be expressed thus,

$$P' = P\frac{\tau''}{\tau_{,,}}\left(\frac{V}{V'}\right)^{2\gamma-1}$$

In the two cases above supposed, P′ = ·59P for the first, and ·52P for the second.

If we would impose a condition that there shall be no negative pressure at the end of the stroke, or that the final pressure shall bear any ratio, expressed by n, to the resistance, we shall have

$$P' = nP = P\frac{\tau''}{\tau_{,,}}\left(\frac{V}{V'}\right)^{2\gamma-1}, \text{ or } n = \frac{\tau''}{\tau_{,,}}\left(\frac{V}{V'}\right)^{2\gamma-1};$$

and if we make $n=1$, we shall have

$\frac{V'}{V} = \left(\frac{\tau''}{\tau_{,,}}\right)^{\frac{1}{2\gamma-1}}$ which limits the expansion to about once and a half the minimum volume, in the cases foregoing; V′ being equal to 1·4 V and to 1·6 V in those cases respectively.

By substituting 2V and 1·5V successively, for V′, in the expression $\left(1-\left(\frac{V}{V'}\right)^{\gamma-1}\right)H$, we find that the fraction of heat converted into available power is ·25, in the first instance, and ·15 in the second. Engines upon this principle will not therefore compare, in point of economy, with those which use compression cylinders. This will be manifest by comparing these results with those given in the March number of this Journal. It is true that, by the use of the regenerator, there may be theoretically a large saving. All the heat absorbed, which is not expended in work-

ing, may, in theory, be taken up again by a regenerator; but it does not follow that all may be again restored to the air. In order to save all the heat, the regenerator should be capable of reducing the temperature, at the close of the stroke, to $\tau_{,,}$; but all which it absorbs between τ' and $\tau_{,,}$ will be lost. Moreover, a regenerator cannot maintain a temperature lower than τ' on its coldest side, so that it will be incapable of alone depressing the temperature sufficiently. All the heat taken in cooling between the limits just named, is invariably lost.

There is, of course, in this form of engine, as in that which employs compression cylinders, a point beyond which a regenerator would be of no avail. This contrivance is confined in its efficiency, to the limits of temperature, $\tau_{,}$ and τ'. If we put these temperatures equal to each other, we shall have

$$\tau_{,} = \tau''\left(\frac{V}{V'}\right)^{\gamma-1} = \tau' = \tau_{,,}\left(\frac{V'}{V}\right)^{\gamma-1} \text{ or } \left(\frac{\tau''}{\tau_{,,}}\right)^{\frac{1}{2(\gamma-1)}} = \frac{V'}{V}.$$

Thus, in case that τ'' is double of $\tau_{,,}$ which will be approximately the case in practice, there will be no advantage derived from a regenerator, when the expansion exceeds, $2\frac{1}{3}V$; but as this is beyond the limit of maximum power, the limitation is of little practical importance.

The difficulty of employing a regenerator with thoroughness at all, is a more serious disadvantage. Stirling was able to pass but a portion of the air through this contrivance: and, as a general rule, where the working piston is in direct contact with the air during the heating process, a certain portion of the mass must escape heating or refrigeration, or must be very imperfectly affected. Moveable regenerators, to take the place of Stirling's plungers, have been suggested by several persons; but besides that their weight would be an objection, they would be less easily kept down in temperature on the cold side, while it does not appear that they have any decided advantage over fixed ones.

There is also great difficulty in applying furnace heat to the air in these engines. This was one of Stirling's most serious troubles; and mainly in consequence of this fact, it is probable that no new attempt will be made to construct an engine strictly on the principle now under consideration.

By a modification of the principle, however, and by employing two supply (or heating) cylinders, in aid of each working cylinder, in one of which the air is in preparation, while in the other it is expanding into the working cylinder beneath the piston, an approach may be made to a realization of the degree of power which theory indicates.

Those who have turned their attention to the planning of engines without compression cylinders, have done so chiefly for the sake of getting rid of what has appeared to them a great evil, in

the resistance of the supply cylinders. But to secure the same power from the same mass of air between the same limits of temperature, on this principle, we must employ a degree of expansion which will produce precisely the same negative pressure at the close of the stroke, which the compression cylinders create. If we work without negative pressure, we do only what can be done in the other form of engine by lengthening the cut-off. And if, in this one, we use air previously condensed, as Stirling did, we do only what Ericsson is doing now. Moreover, if we allow no negative pressure, the range of temperature through which we work, must be very limited.

It is true that the pressure, per square inch of piston surface, will, other things being equal, be in favor of the engine without compression cylinder. But it is a fallacious conclusion to infer that therefore the effective power of the engine will be increased. If we put a to represent the area of the piston, then the length of the stroke will be $\frac{V' - V}{a}$; whereas in the other form the stroke is $\frac{V'}{a}$, and this larger motion is a full compensation, other things being equal, for the less mean pressure. On these accounts, and from the much greater facility of heating and cooling the air on the Ericsson plan, the preference seems to be due to that form of the engine.

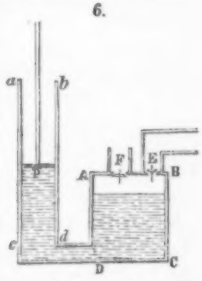
It is to be observed that, in all these comparisons, *the mass of air heated* is supposed to be the same. To compensate for the disadvantages of engines on the Stirling principle, it has been proposed by some to greatly increase the volume of air heated, to make the expansion a less fraction of the volume, and thus to carry a high pressure to the end of the stroke. This is to contradict the principle apparent in all the formulæ above obtained, that true economy requires a large range of fall in temperature in working: for though the regenerator is presumed to compensate for the absence of this, it cannot do so unless, as in Stirling's engine, it acts upon the air in the cylinder itself, or at least without allowing it to expand beyond the capacity of the cylinder—a matter of great difficulty—and at best it furnishes but an inadequate compensation. Moreover, as much advantage is gained by increasing the mass of air heated, in one form of engine, as in another. And since one of the main difficulties is to heat the air at all, it is an unwise expedient to attempt to provide against other disadvantages, by adding to this very serious, and hitherto, on the large scale at least, apparently insurmountable one.

It is perhaps worth considering, whether a more serviceable engine than has yet been invented, might not be made by combining, to a certain extent, the two principles. If two supply cylinders alternately furnish the charge for one working cylinder,

and if each supply cylinder somewhat condenses its charge and holds it exposed to heat, while the other is acting, and finally if the heated charge is not now, as in Ericsson's plan, forced into the working cylinder, but allowed to expand into it, we shall have probably a more thorough heating (or *time* for it at least) than has yet been secured, a higher mean pressure (compensated however by reduced length of stroke) and what is of principal importance, a larger working range of depression of temperature without negative pressure, than either principle alone will furnish. The number of parts would be increased by such a construction, and the machine would become less suited for locomotion than at present; but, as the supply pistons would act alternately, there would be no larger amount of friction to overcome than in Ericsson's.

One of the most serious obstacles in the way of the success of the air-engine, is the difficulty of obviating the effects of the great heat in the working cylinder. This is injurious or destructive to every thing organic, and renders close packing almost impossible. Could a liquid be found, capable of enduring a high heat, this might be made the medium of transmitting pressure, and the working cylinder might be kept absolutely cold. Let *abcd*, for instance, be a cylinder, in which moves the piston, P. Let this cylinder communicate with the larger one A B C D, closed at top, but communicating with an air heater, by the valve E. A B C D being filled with a liquid, and also *abcd*, up to the working piston, air may be admitted to the first, which will transmit its pressure through the liquid to the piston. A valve, F, may then discharge the air. By keeping the bottoms of the vessels, and the connecting channel, cold, the working cylinder will be likewise kept cool. No known liquid, however, would answer this purpose, unless it should be oil; and that would not answer at the high temperatures which have been proposed.

6.



Perhaps the idea is not entirely absurd of filling the chamber A B C D with the fusible alloy of lead, tin and bismuth, which liquefies at about 212° F. The high specific gravity would render great change of level undesirable, and hence A B C D might bear a large ratio in cross section to *abcd;* while the latter cylinder might still contain oil, and the alloy might be chiefly confined to A B C D and the communicating passage.* By this means the temperature of the working cylinder might be kept lower than that of a high pressure steam engine. The tendency of the alloy to oxydize would be an evil which would require to

* Below the range of the piston, however, *abcd* should be equal to A B C D.

be provided against in some manner—perhaps by employing air previously condensed, of which the oxygen had been converted into carbonic acid by passing thoroughly through the fire.

Since the great advantages which the air-engine holds out seem to be so nearly within our reach, and since we seem at present to be debarred from them only by obstacles such as the ingenuity of man has heretofore repeatedly surmounted, it is not only greatly to be hoped, but even to be reasonably expected, that we may soon see the invention perfected, and the important object which has hitherto in a great measure frustrated effort, successfully achieved.

University of Alabama, April 25, 1854.

ART. XXII.—*On the first Hurricane of September* 1853, *in the Atlantic; with a Chart; and Notices of other Storms:* by W. C. REDFIELD.

(Concluded from p. 18.)

West African Hurricanes, and Gales of the Eastern Atlantic between the Tropics.

As the great hurricane whose path we have already indicated, appears to have been of African origin, it may be well to show that the occurrence of storms in this region is not uncommon.

1. A violent hurricane swept over St. Nicholas, one of the Cape Verde Islands, lat. 16° 33′ N., lon. 24° 20′ W., on the second day of September, 1850. Its duration exceeded twenty-four hours; although the chief damage was done in three or four hours, during the morning of that day. All the crops, and nearly six hundred houses, were completely destroyed.* The marine accounts from the vicinity, date this gale on the third; doubtless in nautical time.

The ship *Sir Robert Peel*, for Bombay, after a run of about 120 miles from Bona Vista, encountered this hurricane Sept. 3d, and was completely dismasted.

The *New Margaret* was dismasted in the hurricane on the same day, in lat. 18° N., lon. 25° W.

Ship *Sir Edward Parry*, was in the hurricane Sept. 4th, off the Cape Verde Islands, St. Antonio bearing E. N. E., about 80 miles, [lat. 16° 30′ N., lon. 26° 40′ W]. It came on from eastward, increasing in violence till it blew the masts out of the vessel, while under bare poles.

H. M. S. *Portland*, encountered the gale in this vicinity.

The *Eliza Johnson* was spoken Sept. 20, in lat. 6° N., lon. 22° W., having lost mizenmast and topmasts in the gale, about two weeks before.

* London Times, Feb. 1st, 1851: p. 3.

Most of these vessels put into Rio Janeiro, where these reports were obtained by Capt. Theodore Lewis, from whom I received them in New York.

I can find no reason for doubting the continental origin of this hurricane. Its progression was evidently slow: and its subsequent course is placed under some doubt by the following report from the *Russell*.

The *Russell*, from Salem for Rio Grande, was spoken 24th Sept., lat. 4° N., lon. 20° W., by the *Richard Thornton*, arrived in the Thames from Batavia, and reported having experienced a hurricane on the 6th Sept., in lat. 28° N., lon. 32° W., in which she lost fore-topmast and main top-gallant-masts, boats, &c., also topsails, courses, jib., &c. blown away.

The position and date here given, led me first to lay down the track of this gale as having recurved on a route which passes between Teneriffe and the Azores. But the meteorological observations made by the British consuls at the Azores and Madeira, for the English Government, with other observations collected by Mr. Hunt, Consul General at St. Michaels, which were communicated by the government to Col. Reid, and by him kindly sent to me, do not render this course probable: unless the gale passed near to the Canary Islands, from whence no definite report could be obtained. The route of the gale, therefore, was probably westward; corresponding to Track xxiv. If we suppose the correct latitude to have been 18°, instead of 28°, it will place the *Russell* in a far more probable position, and one which sufficiently coincides with the foregoing reports. The nautical date, however, will then appear about one day in advance; unless the progression of the storm was at the low rate of about five miles an hour The log-book of the *Russell* might solve these doubts. On the westerly course thus indicated, the gale may have passed Bermuda about the 15th of Sept., where there were full indications of the proximity of a slow moving gale. This would show an average progression of between eight and nine miles an hour. Track xxiii.

2. Mr. Piddington has adduced the case of a cyclone passing out from the coast of Africa, to the northward of the Cape de Verdes, on a W. by N., or W. N. W. course, giving to the ship *Devonshire* as she first stood to the S. S. W., and then hove to, about 120 miles westward of St. Antonio, a severe gale from N. E. to South.*

I add here notices of three other gales, in this part of the Atlantic.

3. The *Superior*, from Harbor Grace for Barbadoes, reports as follows: Oct. 14th, 1850, in lat 24° 59′, lon. 47° 10′, experienced

* Piddington's Horn Book for the Law of Storms; 2nd edition, p. 31.

a terrific hurricane, which capsized the vessel at 5 A. M.; cut away both masts, when she righted, and all hands got safely on board again; water eighteen inches above the cabin floor; succeeded in clearing the wreck, and getting under jurymasts.

4. Ship *Damascus*, from Philadelphia for San Francisco, on the 18th of October, 1850, in lat. 25° 58′ N., lon. 41° 19′ W., encountered a severe hurricane, split foresail, main spenser and jib; also blew away main-topsail: after the stormsails were blown away the ship became unmanageable. On the night of the 18th the hurricane moderated.—See the positions on the Chart; marked XXV and XXVI.

The next case, in Sept. 1853, I find in Maury's Sailing Directions, 6th edition; received from the author.

5. The ship *John Wade*, for San Francisco, Sept. 27, lat. 17° 44′ N., lon. 35° 10′ W.; barometer 29·90; wind E., fresh breezes and clear. Sept. 28, lat. 15°, lon. 34° 50′, barometer 29·40; winds E. and E. S. E. First part, fresh breezes; middle part, strong gale. At 8 A. M. hove to under close reefed maintopsail. At 8, barometer 29·60; at 10, 29·7; at 12 M., 29·3. Sept. 29, lat. 14° 32′, lon. 34° 31′, barometer 29·60; winds W., S. S. W. Heavy gale, with violent squalls of wind and rain; middle part, sharp lightning; latter part, moderate; made sail. Capt. Little adds, "I think I was near the track of a hurricane."

The position of this gale appears to coincide nearly with the route of our hurricane of Track XXIV, which was four weeks earlier. The reported directions of wind indicate that Capt. Little crossed the center-path while within the limits of the gale. See XXXI of Chart.

6. To this series may be added a gale or hurricane encountered by Capt. Lavender, in the ship *Roman*, from Canton, Aug. 21, 1832, in lat. 12° 51′ N., lon. 39° 26′ W.: in which, according to Capt. L.'s memorandum,—split the fore-topsail, and scudded five hours under bare poles: ending with cross seas from *N. E.* and *southward*. The center-path of this gale was probably a little south of track XXIV on the Chart.

Other notices of gales in this region have met my eye, in former years; and one shipmaster stated to me that he had encountered, off the Cape Verde Islands, a severe gale of three days duration. This seems to indicate a remarkably slow rate of progression in that gale.

7. Capt. Fitzroy informs us, that on leaving Rio Janeiro for the Cape Verde Islands, early in August 1830, he first steered eastward and crossed the equator far east, which carried him into that tract of ocean between the trades which "in August and September is subject to westerly winds,—sometimes extremely

strong,—and encountered *a very heavy gale;* although so near the equator."* This is likely to have been one of the gales of August which afterwards visited the western and northern portions of the Atlantic, with great severity. Indeed, I strongly suspect this to have been the gale which passed St. Thomas on the 12th, and New York on the 17th of the month; as shown in my first paper on the character and progress of these gales.†

Capt. Fitzroy states, also, that at Port Praya, [lat. 14° 53′ N., lon. 23° 30′ W.,] no vessel should deem the bay secure during July, August, September and October,‡ because southerly gales sometimes blow with so much strength, and the rollers sent in by them are so dangerous to ships; and having experienced the force of these gales in the vicinity of the Cape Verde Islands, he confidently warns those who are inclined to be incredulous about a gale of wind being found in 15° of north latitude; beyond the [supposed] limits of the hurricane regions.§

8. If from this inter-tropical field we extend our inquiries northward to the Canary Islands, in lat. 28°, near the African coast, we may learn of other active cyclones that have crossed these Islands, in pursuing their orbital course to the shores of northern Africa and southwestern Europe. The route of one of these storms which passed near the Island of Madeira in October 1842, as shown by Col. Reid, is seen on the Chart.‖

9. I find record of another great storm, which passed over the Island of Teneriffe, on the 6th of November, 1826.

Track XVIII, seen further westward on the Chart, is the inferred route of a severe hurricane, in 1828, which was reported to me by Capt. Corning: long known as an intelligent merchant and navigator.

These several cases, together with Col. Reid's Bermuda hurricane of Sept. 1839,¶ the track of which is seen on the Chart, and that of Capt. Maclean of Sept. 1853, of which reports are annexed, are submitted as indicating the general course of progression of inter-tropical cyclones, in the eastern Atlantic; and their occasional identity, as well as systematic conformity, with those which visit the more northern portions of this oceanic basin.

* Voyage of the Adventure and Beagle, (Surveying vessels), vol. i, pp. 1 and 3.

† This Journal, First Series, vol. xx, p. 34–38.

‡ These are the months which constitute the "hurricane season" of the Windward Islands of the West Indies, where, as we have formerly shown, the hurricanes arrive from a more eastern portion of the Atlantic. We have now, more than presumptive evidence of their African origin.

§ Voyage of Adventure and Beagle, vol. i, p. 53.

‖ See Col. Reid's Progress of the Development of the law of Storms, p. 275—279: Where is found also an account of a gale in the S. E. part of the Mediterranean.

¶ For a full account of this hurricane, see Col. Reid's Attempt to Develope the law of Storms: 2nd Edition, p. 444–448.

CAPT. MACLEAN'S HURRICANE, OF SEPTEMBER 27th, 1853.—In passing over the several violent hurricanes of the past Autumn, of which I have more copious notices, I select only the present case, because its recurvation was eastward of Bermuda. A good account of this storm is given in the London Shipping Gazette of November 8th, by Capt. Maclean, who had studied the cyclones, and was thus well prepared to meet their emergencies.

His ship, the *Gilbert Munro*, left the Island of St. Lucia on the 8th of September, and lost the trade wind on the 13th, in lat. 24° 33′ N. Light winds followed, with a high barometer, till on the 26th the weather became dark and gloomy, and the wind veered to E. S. E. and S. E. At noon, in lat. 33° 10′, lon. 59° 07′, the aneroid barometer had fallen $\frac{2}{10}$ths, and the mercurial barometer began to sink also. In the night following, the wind, at S. E., increased to a fresh gale, with squalls: [Being under the right limb of the gale, then near its point of recurvation.] At 4 A. M. of 27th the wind abated; but as the morning advanced it again freshened, from S. S. E., and the bar. had fallen $\frac{4}{10}$ths; at 10 A. M. hard gale, and bar. still falling; made the necessary preparations, being certain, from the direction of the wind, that the center was to the S. W., if a rotary storm, and would soon overtake us, in its progress northeastward, and that we should then have the gale from an opposite point.

At noon of 27th heavy gale at S. S. E., and heavy sea; lat. 35° 19′, lon. 56° 36′; rain fell in torrents till 1·30 P. M., when it ceased; barometer falling rapidly. Soon after there was a lull, and in ten minutes a full calm. Being now certain of an opposite wind, had but just time to prepare for it, when it burst upon us with increased fury from N. W., veering afterwards to N. N. W. and N. N. E. At 2 P. M. it blew a perfect hurricane, with dangerous cross sea. At 2·30 P. M. the ship was blown on her beam-ends; but with great exertions was payed off before the wind, and run admirably. It continued to blow with great violence till near midnight; when the wind backed to N. N. W., the barometer rising; and at daylight of 28th had abated to a common gale. At 8 A. M. more moderate.

Capt. M. commends a knowledge of the law of storms to every shipmaster and nautical man.

The brig *Samuel and Edward*, reports having experienced the hurricane Sept. 28th, lat. 34° 40′, lon. 56° 20′, from S. to N.; lost sails; &c., and lay ten hours under bare poles.

The Schooner *Werada* took the gale in lat. 35°, lon. 59°; and while scudding under close reefed sails, was taken aback by the hurricane from N. W.

At Bermuda, lat. 32° 15′, lon. 64° 40′, heavy rains at this period, with a very strong N. E. gale [force marked 10], from about noon of 26th to evening of 27th, veering to N.; thus showing

the left side of the cyclone. Barometer at 4·30 P. M. of 26th, 29·72. At 7·30 A. M. of 27th, 29·84.* See Track xxx of the Chart.

The foregoing notices of storms of inter-tropical origin in the eastern Atlantic, may serve to show their analogies and relations to those previously traced in the western Atlantic, and in the North American states. Let us now pass westward in the same parallels, to the nearer portions of the Pacific Ocean.

Gales of the Eastern Pacific, near the Mexican Coast.

Our direct knowledge of the paths of these gales is necessarily limited; but the interests of an increasing commerce, as well as of meteorological science, claim the notices which follow.

1. The *Joseph Butler*, on or about the 24th of June, 1850, encountered a severe gale of wind, near lat. 16° N., lon. 107° W., [260 miles from the shore of Mexico,] which carried away her mainmast. I have no further accounts of this gale.

2. The barque *Como*, on the 5th of August, 1850, in lat. 14° 20′ N., lon. 117° W., encountered a severe gale, commencing at N. and veering to W. and south. Lost sails and bulwarks, and sustained much other damage. These winds denote a course of progression corresponding to that of the hurricanes in the West Indies, and that the vessel was in the left side of the storm-path.

3. Niagara's Hurricane.—The *Niagara* was dismasted in a hurricane Sept. 9th, 1850, about ninety miles south of Acapulco: [lat. 15° 16′ N., lon. 99° 50′ W.]

The *Diana*, Sept. 11th, lat. 22° N., lon. 116° W., had a severe hurricane from N. E., veering to S. W.; blew five hours; vessel hove on beam-ends.

The Diana's position was in the left side of the storm-path, but near to the axis line; the progression of the storm being still northwesterly. Its course of progression from the *Niagara* was 34° north of west; or W. N. W., nearly. Its rate of progress was nearly twenty-three miles an hour; allowing no error for the nautical dates. Part of the track falls on our Chart. See Track xxviii.

4. The *Laura*, Sept. 26, 1850, lat. 26° N., lon. 123° W., in a severe gale was thrown on her beam-ends; lost cargo, &c. I have no further account of this gale.

5. The *Kingston*, from San Francisco for Panama, experienced a severe gale on the Mexican coast, and was thrown on beam-ends, Oct. 1, 1850, in lat. 14° N.; and reports that the gale swept the whole coast with great violence; as may be seen in the succeeding statements.

The *Belgrade*, from San Francisco for Realejo; Oct. 1, fine breeze from W. N. W., and *heavy swell from S. E.* At 10 P. M.

* From Signal Station Reports in Bermuda Gazette.

wind hauled suddenly to S. E., with increased force and squally appearances; at midnight under single reefed topsails; 1 A. M. still increasing, with vivid lightning and heavy rain; 4 A. M. split fore-topsail; 8 A. M. lost foresail; gale increasing to a hurricane; thrown on beam-ends, with loss of main and mizen-topmasts, with head of mainmast, when the ship righted a little. At 1 P. M. Oct. 2nd, hurricane still increasing, ship on her beam-ends; lost fore-topmast, with much other damage; at midnight, blowing as hard as ever; at 4 A. M. Oct. 3d, more moderate, heavy rain; Oct. 4th, lat. 18° 11′ N., lon. 104° 5′ W., made for Acapulco. It may be seen that this vessel was on the right of the axis path of the storm.

The *Galindo*, on the same route, experienced a severe hurricane on the 1st and 2nd of October; was thrown on beam-ends and dismasted; and arrived at Acapulco at the same time with the *Belgrade*.

The *Lovina*, off Cape San Lucas, the southern point of California, Oct. 5th, was thrown on beam-ends in a violent hurricane, and lay twenty-one hours.

The *Fanny*, from Mazatlan, in the gulf of California, for San Francisco, was damaged in the gale, on the 5th and 6th of October, and put back to Mazatlan.

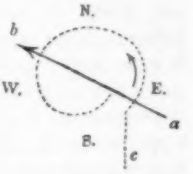
The progress of this hurricane, during four days, appears to have been N. W. by W., nearly; at a rate not exceeding eight or ten miles an hour. Part of this track falls on our Chart: Track XXIX.

6. Amazon's Hurricane.—The brig *Amazon*, from New York for San Francisco, encountered a severe hurricane Oct. 3d, 1850, in lat. 13° 30′ N., lon. 116° 50′ W.; which commenced at S. W. veering successively to S. E.; E.; N.; W.; ending at S. W.; in which lost main-topsail and foresail. Capt. Watt states that the gale was equally severe as those in the West Indies. This off-shore hurricane was cotemporaneous with that last noticed. The following is drawn from the account which was published by a passenger of the Amazon.

Oct. 4th, lat. 13° 40′ N., lon. 116° 30′ W.: last night the brig encountered a hurricane, preceded by squalls from S, W., with heavy rain. The squalls increased in number and intensity, until 5 P. M., when the hurricane commenced; brig under close-reefed fore-topsail and mainsail. Capt. Watts put his vessel before the blast, or "scudded" her. The tempest raged during the night, with momentarily increased fury. It veered from S. W. to due south, thence to S. E., and thence to N. E. and north, and from thence to S. W., thus making the circuit of the compass! According to our reckoning, it veered thirty-four points in the space of six hours; during which time the brig was kept before it, in which lay our only chance of escape. At 4 A. M. the

foresail was blown from the yard, and the vessel was then brought to the wind, but could not withstand the tornado, and was blown directly down on her side, or beam-ends. Apprehending she would founder, the order was given to put her again before the wind, but the attempt was unsuccessful. As a last resource, the main-topsail was let go, when she paid off, and dashed away like lightning before the tempest. She was kept scudding till the hurricane ceased and was then laid to in a heavy gale from S. W., which followed the hurricane.

From the above we may infer that the cousre of the vessel while scudding, was not unlike that shown in the annexed figure.



a b Course of the storm's progression.
c Course of the *Amazon* in the gale.

The short time in which the brig ran entirely round the axis of the gale, after entering its violent portion, shows that its diameter was small; and that its progression was remarkably slow. This slowness is also shown by the manner in which the brig, steering N. for San Francisco, was able to *overtake* the cyclone, and run into it, upon its southeastern side, where its wind was southwesterly. Hence, too, after clearing the vortex of the cyclone, and heaving to, the duration of the exterior portion of the gale was so much prolonged, notwithstanding the drift of the vessel was in a direction opposite to the progression of the storm. It is probable that this progression did not exceed four miles an hour; and it may have been less.

This is a slower rate of advance than I have yet found on the Atlantic; but it accords well with other cases which have occurred within the tropics, in the Indian Ocean. It appears, also, as having some relation to the slow rate of advance already noted in the cotemporaneous in-shore hurricane of the *Kingston*. Hence, we may infer, that the great current of rotation in which the cyclones are imbedded was at this period and in this region, at least, comparatively sluggish and inactive. We have noticed a similar condition in the Eastern Atlantic, in the previous month; in the case of the Cape Verde hurricane, of Track XXIII.

7. CAPT. BUDD'S GALE, OF OCT. 1851.—Capt. Budd's steamer from San Francisco, for Panama, was on the 21st of October in lat. 22° 07′, off Cape San Lucas. At daylight of 22d the wind was very high, hauling to S. E., preceded by *a heavy swell from the same quarter*. The gale blew heavy from S. E., and then commenced hauling to N. E., and blew still more heavy: barometer 29·75. He had now crossed the entrance of the Gulf of California, to within sixty miles of Cape Corientes. At 4 P. M. gale abating, and hauling to the westward, going round by the north.

The winds in this case appear to indicate that Capt. Budd fell under the right hand or northern side of the gale, as it first ap-

proached; and that the gale recurved northward, upon the contiguous portion of Mexico, before the axis of the storm had reached the position of the ship. See Chart.

8. Panama's Gale, of July 1852.—The *Panama*, experienced a hurricane July 16, 1852, in lat. 15° N., lon. 115° W.; which lasted ten hours: carried away top-gallant-masts, yards, sails, &c.

Extract from logbook of ship *Empire*, bound for San Francisco: July 19th, 1852, commences with heavy gales and bad sea, from the north; under double-reefed topsails and courses: [ship in front part of the gale, to the left of its axis path]. At 8 P. M. heavy gale from N. N. W.; at 10 P. M., very heavy gale; hove the ship to under triple-reefed main-topsail; midnight, gale increased to a hurricane; the mainmast went by the board, together with the mizenmast, fore-top-gallant-mast, &c., with every thing attached; blowing a complete typhoon. At 5 A. M., succeeded in clearing the wreck; at 7 A. M. gale had in some measure abated, and at 8 A. M. got the ship before the wind, then blowing from S. S. W.; at noon of 20th, only a brisk gale from S. S. E. Lat. by account, 17° 4′ N., lon. 117° 35′ W.

This could have been none other than the Panama's gale, moving on a course between 30° and 40° north of west; and, if there be no error in the Panama's date, at the rate only of about three miles and a half per hour!

This slowness of progression, in the three hurricanes of the *Panama*, *Amazon*, and *Kingston*, is of great interest for navigators in the Pacific. For it shows how perfectly the exposure and safety of their vessels, during such hurricanes, are placed in their own control; at least, in cases where sea room on all sides is afforded them. Thus, if the master of the *Amazon* had comprehended the character of his hurricane, or its law of rotation and progression, he might have run more eastward until the state of the barometer and winds would have allowed him to come up to his desired course. This would have enabled him to make a safe, rapid, and successful run, towards his port of destination, while he kept in the outskirts of the gale.

The *Empire*, when headed off by the north wind in the front of the gale, could not pursue her course for San Francisco, nor safely heave to, on either tack. But she had opportunity to run southward in the beginning of the gale, keeping the wind on the starboard quarter, until the state of the barometer and the diminished strength and westerly changes of the wind should enable her to turn eastward, around the rear of the hurricane, and thus regain her course with a fair wind.

9. A violent hurricane occurred at Cape Corientes and Ipala on the night of October 11th 1853; in which the *Eclipse*, a valuable ship, was totally lost, about five miles east of Ipala: [in lat.

20° 10′ N., lon. 105° 25′ W.] It first blew off the land, from the northward, and shifting suddenly to the westward, blew a perfect hurricane, right on shore. This may indicate its recurvation near the southern entrance of the Gulf of California at Cape Corientes. It has been shown that some hurricanes of the gulf of Mexico, commence their recurvated course to the northward and eastward in a still lower latitude. For such a case, see this Journal, vol. i, New Series, p. 153–162.

The inter-tropical gales of the North Pacific which are comprised in these few notices, are seen to have occurred in the several months from June to October, both inclusive; and I have now before me an account of another violent gale, far to the westward, in the month of May. The prevalence of storms on that coast in the other months, from October to April, has been noticed by Humboldt and other writers; and is now but too well known by the experience of navigators.

We thus establish the prevalence of violent cyclones upon the southwestern coast of North America at all seasons of the year: and find that these are sometimes of great violence. That many of these cyclones pass over the Mexican territories, some to the gulf of Mexico, under the local name of *northers*, and others to the territory of the United States, I can find no reason to doubt.

The very prominent characteristic of southeast winds, in the storms which commonly visit the Pacific coast, affords evidence of their progress along the coast in the lower latitudes, and of their direct entrance upon those shores in higher latitudes, subsequent to their northwardly recurvation. These characteristics early attracted my attention, in the gales which are noticed in the voyages of Cook, Vancouver, and others, and in the Journals of whalers, which came under my inspection.

We might infer, therefore, without reference to other and direct evidence, that the same general system of cyclonic movement prevails on the continent of North America that is found on the Atlantic. Indeed, a glance at our storm Chart might afford conviction of this fact.*

A competent knowledge of the cyclones and of the law which governs their development, has become essential to our navigators. Both merchants and insurers are beginning to discover that even the good qualities of a vessel have commonly less influence upon the safety of her voyage, than has the intelligence and skill of the commander. Hence, there are now insurers who freely select those risks which are in charge of the most compe-

* In almost every region of the Pacific, violent cyclones are known to occur: and even within five or six degrees of the equator, the ravages of a hurricane at certain islands have occasioned the destruction of a large portion of the native population.

The results of the recent inquiries have now shown, by direct observations, the prevalence of the cyclonic system of storms entirely around the globe, in both hemispheres; excepting some interior or inaccessible portions of the old continent.

tent masters; leaving other risks of whatever class, to underwriters who are willing to rely on the classification of the vessels.

American Storms of December 1836.

From the 30th day of November to 21st December, 1836, six great cyclones passed successively over the United States; having passed New York on Nov. 30th,—Dec. 5th,—10th,—14th,—17th, and 21st, respectively: under which, my barometer fell ·62,—·35,—·44,—·86,—·90, and 1·05 in., in the several cases.* The surrounding waves of exterior pressure raised by their rotation, and separating each cyclone from the other, were indicated by my barometer as follows, viz.: Nov. 28th, 30·27; Dec. 4th, 30·29;—8th, 30·35;—12th, 30·28;—16th, 30·45;—19th, 30·80; and Dec. 22nd, 30·72 inches. Each cyclone exhibited here the winds of its two right quadrants, gradually veering, from a southern quarter to the western board, as it went onward; thus showing the cyclonic centers to have passed far westward of New York, and over the Canadas, in their several routes to the northern regions of the Atlantic.

In the last of these storms, which has been examined by Prof. Loomis,† the wind at New Orleans, on the 20th, blew hard from a southern quarter, and also on our Atlantic coast, during the latter part of 20th and early part of 21st; veering westward. At Rochester, N. Y., it blew from southeast on the afternoon of 20th, with great power, and furiously at Buffalo, also veering round by the south to the west, during the night; thus showing that the axis of this gale passed northwardly at a distance much to the west of these places. This fact is confirmed, also, by the reports of winds as made to the Regents at Albany, and by those obtained from the military posts and other sources; very many of which are given by Prof. Loomis. The same fact is shown by barometric observations as published by him. For although the central nucleus of the storm, or area of greatest barometric depression, passed the western observers during the night, when the greatest and most rapid fall and rise of the barometer was not noted, yet, the depression as recorded proves to be greatest as we go towards the true center-path of the storm, as the same is approximately indicated on the Chart: marked XXVII. This is seen in the observations made at Lexington, (K.) Springfield, (O.) Marietta, Twinsburg, Rochester, Syracuse, Albany, Montreal, Hanover, and Quebec; which, even as given, show a mean fall of 1·075 in.: while those of twelve places on or near the Atlantic border, from Savannah to Newfoundland, show a mean fall of

* This series may serve to illustrate the continued succession of cyclones in the United States.

† Transactions of the American Philosophical Society, vol. vii, New Series, p. 125–164.

but ·91 in. If the true course of the storm had been from west to east, the fall in the barometer would have been much the greatest on the Atlantic border; owing to the lower level, which is not considered and allowed for in the above estimate, and to a less obstructed rotation of the storm, on reaching the Atlantic.*

Moreover, the barometric minimum was observed at Quebec about as early, on the 21st, as at New York and its vicinity; although 420 miles further to the north, and nearly on the same meridian. This more rapid advance of the central portion of the storm, which has been seen in other cases, proves that the true course of progression was on the general route which I have indicated. The rate of the storm's progression, from noon of 20th to noon of 21st, I estimate at about 33 miles an hour.

It is true, however, that Prof. Loomis has traced this storm *eastward*, from the Mississippi to the Atlantic; and has stated, also, that in this case "there was no whirlwind." But, not rejecting his claims as a cyclonologist, I may state that he had almost no observations other than from the right side of the storm's center-path. Now, in like limited manner, and *with like directions and changes of the wind*, the great hurricane of September last, which we have just considered, may be traced eastward from its center-path, for a greater distance, and in the same latitudes, as already shown. The like is also true of the great Cuba hurricane of 1844, which was examined in this Journal;† as also, of Col. Reid's hurricane which crossed Bermuda and Newfoundland, in Sept. 1839, (see Chart,) on perhaps the most northerly course that has been traced in any storm.

Yet, who that duly examines these cases, will doubt that these storms, in their essential character, were great whirlwinds, moving northward and eastward? Indeed, the same or like phenomena may be traced in every great cyclone that passes over these latitudes. This eastward extension appears due, in part, to the enlargement of the cyclone; and while affecting its external form, and that of the lines of equal pressure, it does not essentially change the rotative movement; as may be seen by the continued development of the cyclonic winds, and their influence on the barometer.

It is well known that other and similar tracings from west to east have been made of the progress of various storms in the

* The extreme range of the barometer in a period of seven years at Hudson, O., near to Twinsburg, and about 1100 feet above tide, as given by Prof. Loomis, is 1·719 in.: while the range observed in New York during the same period, was 2·25 in. Difference, ·531. The mean of the annual ranges at Hudson during the same period appear to have been 1·402 in.: while the mean of the annual ranges at New York was 1·874 in. Difference, ·472 in. It appears, therefore, that near half an inch should be added to the depression of the barometer in some of the western observations of this storm, in order to a fair comparison of the barometric indications with those on the Atlantic border in the same storm.

† This Journal, vols. i, and ii, New Series, 1846.

United States. It is believed, however, that the clew to these cases is already afforded; and that many or most of these storms were true cyclones; with orbital courses really analogous to those which are seen on the Chart.

WHAT ARE CYCLONES?—The term Cyclone was first proposed by Mr. Piddington, to designate any considerable extent or area of wind which exhibits *a turning or revolving motion;* without regard to its varying velocity, or to the different names which are often applied to such winds. If used in this sense it may prevent the confusion which often results from other names, more variable or indeterminate in their signification. Thus, all hurricanes or violent storms may perhaps be considered as cyclones or revolving winds. But it by no means follows that all cyclones are either hurricanes, gales, or storms. For the word is not designed to express the degree of activity or force, which may be manifested in the moving disk or stratum of rotating atmosphere to which it is applied. It often designates light and feeble winds, as well as those which are strong and violent.*

It follows that the local directions and changes of the wind in any cyclone, and their effect on the barometer, are much like those exhibited in the gales and storms of the same region, except in the *degree* of their effect; which is doubtless proportioned to the general activity of the rotation, integrally considered.

The cyclones are often productive of rain in a portion of the cyclonic area; but vary in this respect, in different regions, and at different seasons of the year.

UNIVERSALITY OF CYCLONES.—As early as 1833 my inquiries led me to announce the conclusion that the ordinary routine of the winds and weather in these latitudes often corresponds to the phases which are exhibited in the revolving storms, already de-

* As regards the temperate latitudes of the northern hemisphere, the true normal wind is commonly from the southwestern quarter of the horizon; and the accession of a cyclone, except on its right margin, is usually marked by a change of the wind from the western board to some point on the eastern side of the meridian, accompanied and often preceded by a fall of the barometer. On the right margin of its path the cyclone may commence from near the southwest, in perfect continuance with the normal wind with which it here coincides. As the cyclone advances, the wind on the right of its axis-path *veers* "with the sun," or from the east towards the south and west; while on the left side of this path or line, the same cyclonic wind changes from the east to the *north*. On and near the axis-path the earlier winds of the cyclone blow *across* the line of progress, from the southeastern quarter to the northwestern, with a falling barometer; and when the axis of the cyclone has passed, its later winds are found crossing the line of progress in the opposite direction, from the northwestern quarter to the southeastern, with a rising barometer.

The true cyclonic wind may not always be found at the earth's surface, in every portion of the path of the cyclone, if its action be feeble, or subject to interruption, or to the interposition of bordering winds or cyclones. Even in stormy cyclones, irregularities of direction are often noticed at the surface; but in these cases it commonly happens that the *storm-scud*, at the elevation of a few hundred feet, exhibits locally the true direction of the cyclonic wind. But the changes of direction successively observed in the storm-scud, are commonly *in advance* of those in the lowest wind.

scribed, and that a correct opinion, founded upon this resemblance, can often be formed of the approaching changes: and that the variations of the barometer resulting from the mechanical action of circuitous winds and the larger atmospheric eddies, pertain not only to the storms, but to a large portion of the winds in these and the higher latitudes. *Vide* this Journal for October, 1833, (vol. xxv,) pages 120 and 129.

The more inert and passive cyclones which seldom gain attention, but which constantly occupy in their transit the greater portion of the earth's surface, appear to move in orbits or courses corresponding with those of the more active class which have been traced on the storm-charts; a result that will not be doubted by those who have given careful attention to this branch of inquiry. In a broad view of the case, the constant occurrence and progression of the cyclones, in various degrees of activity, constitutes the normal condition of the inferior or wind-stratum of the atmosphere, at least in the regions exterior to the trade winds of the globe; to say nothing of their prevalence in the intermediate region, where their presence is shown on some occasions by the most indubitable evidence.

At the late meeting of the American Association for the Advancement of Science, held at Cleveland, an ably elaborated paper was presented by Prof. James H. Coffin, of Easton, Pa., on the relations which exist between the direction of the wind and the rise and fall of the barometer. By a careful analysis of these effects during all seasons of the year, as observed at various places in the north temperate zone, Prof. Coffin establishes the northeastwardly progression and leftwise rotation ↺ of a continued series of cyclones, in which are developed the same local relations between the rotary action of the various winds and the movements of the barometer that are found in the several rotary storms and hurricanes which have been subject to investigation; and such in kind, though not in degree, as we have seen in the principal case already considered. Thus, if I rightly appreciate the labors of Prof. Coffin, the cyclonic character of the variable winds in the temperate latitudes, which had been inferred from special observations and an extended range of geographical inquiry, is now established by a different and wholly independent method of investigation.

The storm-paths and routes of the cyclones clearly indicate, also, the true course of the principal circulation in the lower atmosphere, on both sides of the equator. An enlarged view of these physical truths and conditions may serve to convince meteorologists and others of the necessity for a thorough revision and correction of the received views of dynamical meteorology. Such revision, I apprehend, is now imperatively required. For the constant recession from the equator of a great portion of the

lowest currents of atmosphere, as seen in the orbital courses of storms in all latitudes, and to which I have already alluded, together with the mean direction of the observed winds in the northern temperate zone, even neglecting other world-wide phenomena, may suffice to show, that the current theory or hypothesis for explaining the general winds of the globe, is essentially erroneous and defective in its application, and greatly obstructs the path of scientific inquiry.

New York, March 18, 1854.

ART. XXIII.—*Researches upon Arseniuretted and Antimoniuretted Hydrogen, and their relations to Toxicology;* by RAPHAEL NAPOLI, Royal Professor of Chemistry at Naples.

(Read before the American Association for the Advancement of Science, at Washington, May, 1854, by T. S. Hunt, for the author.)

AFTER Lassaigne had observed that nitrate of silver decomposes arseniuretted hydrogen with the formation of arsenious acid, and the separation of metallic silver, Jacquelain proposed the chlorid of gold for the same object, and Berzelius in his *Traité de Chimie* says of this gas, "It precipitates the precious metals, as gold and silver, from their solutions, and is itself dissolved by the oxydation of its elements." Such a decomposition really takes place with arseniuretted hydrogen and the ter-chlorid of gold, and also with the ferric and platinic chlorids; still no chemist, so far as I know, has explained the reaction.

The explanation which I now propose was suggested to me by M. Nicolo Prestandrea of Messina, who wrote to me as follows; —"When arseniuretted hydrogen is passed through a solution of chlorid of gold, we see that the gold is really reduced, and the arsenic dissolved as arsenious or arsenic acid. But as these bodies contain no oxygen, whence comes this element to oxydize the arsenic, unless from the decomposition of the water of the solution, whose hydrogen at the same time forms hydrochloric acid, with the chlorine of the gold salt? This acid might be formed from the union of this chlorine with the hydrogen of the gaseous arseniuret, without any decomposition of water, in which case both gold and arsenic, should be separated in the metallic state, according to the following equation,

$$Au\,Cl_3 + As\,H_3 = 3H\,Cl + Au + As.$$

So that the theory of this reaction is not yet made clear. In order to explain the facts just mentioned we must suppose that arsenic, in its nascent state at least, can be dissolved by hydrochloric acid; such being the case, it would be easy to understand the formation of the acids of arsenic, the precipitation of the gold,

and the production of hydrochloric acid. The gold having been reduced, according to the formula just given, there remains 3HCl, and As, which would yield $AsCl_3$, and three equivalents of free hydrogen. The chlorid of arsenic when diluted with a large quantity of water, is decomposed into arsenious and hydrochloric acids."

On consulting Berzelius and other works of authority, I found it stated on the one hand, that arseniuretted hydrogen is not altered by hydrochloric acid, and on the other, that arsenic is not affected by hydrochloric acid. These statements seemed to render the proposed explanation inadmissible, but I have found by experiment that they are incorrect, and have arrived at the following conclusions, first, arseniuretted hydrogen is almost totally decomposed by pure concentrated hydrochloric acid, and secondly, arsenic itself is soluble in this acid.

I passed the arseniuretted hydrogen gas generated in Marsh's apparatus, through concentrated hydrochloric acid in a Liebig's bulb tube, and after continuing the process for an hour, chlorid of arsenic was found dissolved in the acid, thus proving the decomposition of the arseniuretted hydrogen.* To prove the second point I took some crystallized metallic arsenic, washed it repeatedly with cold and pure hydrochloric acid, and when its surface was perfectly free from oxyd, attacked it with boiling hydrochloric acid in a small retort, placing a little water in the receiver. On collecting the portion which distilled over, I found it to contain chlorid of arsenic, and the liquid residuum in the retort, contained a notable portion of the same chlorid; thus showing the solubility of arsenic in hydrochloric acid.

These facts, being well established by a series of repeated experiments, gave me an explanation of the reactions of arseniuretted hydrogen on the perchlorids of gold and iron, and the bichlorid of platinum; the formulæ are as follows:

$$AuCl_3 + AsH_3 = Au + AsCl_3 + H_3.$$
$$3Fe_2Cl_3 + AsH_3 = 6FeCl + AsCl_3 + H_3.$$
$$3PtCl_2 + 2AsH_3 = Pt_3 + 2AsCl_3 + H_6.$$

While studying the above reactions, I had occasion to repeat some observations which were made long since by Stromeyer, and appear to have been forgotten by chemists, but which serve to render more accurate the examinations for arsenic and antimony by Marsh's apparatus. The facts are these: arseniuretted and antimoniuretted hydrogen are both decomposed by pure and

* The decomposition of arseniuretted hydrogen with hydrochloric acid, is represented by $AsH_3 + 3HCl = AsCl_3 + 3H_2$, and is analogous to that of hydrid of copper with the same acid; in each case a metallic chlorid is formed, and the hydrogen of both compounds is set free. See Brodie's remarks on the latter reaction, in the Chemical Gazette for 1853, p. 300.—(T. S. H.)

highly concentrated nitric acid, the former yielding water, nitrous vapor, and soluble arsenic acid, while the latter gives insoluble antimonic acid, causing a turbidness in the liquid, which is increased by concentration. If then we pass a mixture of these two gases into nitric acid, and afterwards boil the turbid solution, a yellowish-white precipitate of antimonic acid separates, and by adding water and continuing the ebullition, the antimony is entirely thrown down, while the arsenic acid remains dissolved.

The two gases are also decomposed by aqua-regia, with the formation of chlorid of arsenic, and perchlorid of antimony. By a careful distillation of the mixture, the arsenical chlorid passes over first, while the chlorid of antimony remains in the retort, as has been shown by Malaguti and Sarzan. An analogous reaction is produced with strong hydrochloric acid, but with this difference that the arseniuretted hydrogen is almost entirely decomposed, while the antimonial gas undergoes a less complete decomposition.

The applicability of these reactions to the concentration and separation of the arsenic and antimony, in the gas obtained by Marsh's apparatus, will now be apparent. Instead of burning or decomposing by heat, the evolved gas, it is passed through a U tube to dry it, and then by means of a caoutchouc connector into a Liebig's bulb apparatus, containing concentrated, fuming nitric acid, which is to be heated by immersion in a bath of water or oil. Having assured ourselves of the purity of the gas evolved by the zinc and sulphuric acid, we add the suspected matter, and then pass the gas into the hot nitric acid, which completely decomposes in the manner just described, any compounds of arsenic or antimony which may be evolved. If the nitric acid remains clear, we are almost sure of the absence of antimony. When the operation is finished, the contents of the bulbs are to be transferred to a small flask, the apparatus washed out with a little nitric acid, and the whole carefully evaporated to one-half. If there is no precipitate, the absence of antimony is certain; we then evaporate still further to remove the excess of acid, dilute the residue with water and examine it like a pure solution of arsenic acid. Should the nitric acid appear turbid either before or after evaporation, antimony is present, and perhaps arsenic; in this case, after evaporating as before to a small bulk, we add water and filter, the antimony remains behind in an insoluble condition, while the arsenic, if any were present, is held in solution, and both can be examined by the ordinary tests.

In the case of a mixture of arsenic and antimony we may proceed in a different manner for their separation. Having prepared a small tubulated retort, to which can be adapted a small receiver, we introduce through the tubulure, a tube reaching nearly to the bottom of the retort, in which is placed a small quantity

of aqua-regia composed of two parts of hydrochloric and one of nitric acid. The retort being gently heated, the gases from Marsh's apparatus are passed in a slow current into the aqua-regia. In the reaction which takes place, any arseniuretted or antimoniuretted hydrogen will be decomposed, and chlorid of arsenic or antimony formed. This operation finished, the tube is removed, the tubulure closed, and the receiver, partly filled with water, being attached, the acid liquid in the retort is gently distilled to one-half its volume. We then examine the water of the recipient, and if any arsenic is present, it will be found in the distilled liquid; if this metal be absent, we find nothing in the water, or at most, some traces of antimony, in case the operation has not been well conducted. If the gas contained any antimony it will all be found in the retort, in the state of perchlorid.

I have not deemed it necessary to present the numerical results, which in repeated experiments have shown the great accuracy of these methods, but believing that chemists will at once recognize the value of the proposed processes, it is sufficient for me to have called their attention to the following facts, in part already known, and in part new.

1st. The power of hydrochloric acid to dissolve and decompose arseniuretted hydrogen.

2d. The solubility of metallic arsenic in the same acid, when concentrated.

3d. The explanation of the reactions of arseniuretted hydrogen, with the perchlorids of gold and iron, and with the bichlorid of platinum.

4th. The decomposition of arseniuretted and antimoniuretted hydrogen by nitric acid, and by aqua-regia.

5th. The application of these reactions to toxicological analysis, for the detection and separation of arsenic and antimony.

ART. XXIV.—*On some of the Crystalline Limestones of North America;* by T. S. HUNT, of the Geological Commission of Canada.

(An Abstract of a paper read before the American Association for the Advancement of Science, at Washington, April, 1854.)

THE crystalline limestones of Canada, with those of New York and the New England States, may be divided into four classes, belonging to as many different geological periods. The first and most ancient occur in that system of rocks, named by Mr. Logan the Laurentian series, which extending from Labrador to Lake Huron, forms the northern boundary of the Silurian system of Canada and the United States. The lowest beds of the Silurian

repose horizontally upon the disturbed strata of this oldest American system, a southern prolongation of which crosses the Ottawa near Bytown, and the St. Lawrence at the Thousand Isles, and spreading out, forms the mountainous region of northern New York. This series consists in large part of a gneiss, which is often garnetiferous; but beds of mica slate, quartz and garnet rock, hornblende slate and hornblendic gneiss are also met with, besides large masses of a coarsely crystalline, often porphyritic rock, consisting chiefly of a lime and soda feldspar, which is sometimes labradorite, and at others andesine, or some related species, and is generally associated with hypersthene. It often holds beds or masses of titaniferous iron ore, and from its extent, occupies a conspicuous place in the series. It is the *hypersthene rock* of McCulloch and Emmons.

With these, the limestones are interstratified, but their relations to the formation have not yet been fully made out. All of these rocks bear evidences in their structure, that they are of sedimentary origin, and are really stratified deposits, but their investigation is rendered difficult by the greatly disturbed state of the whole formation. Among these stratified rocks, there are however dykes, veins, and masses of trap, granite and syenite, often of considerable extent, which are undoubtedly intrusive. There are abundant evidences that the agencies which have given to the strata, their present crystalline condition, have been such as to render the limestone almost liquid, and to subject it at the same time to great pressure, so that in many cases it has flowed around and among the broken and often distorted fragments of the accompanying silicious strata, as if it had been an injected hypogene rock.

The limestone strata are from two or three feet to several hundred feet in thickness, and often present a succession of thin beds, divided by feldspathic or silicious layers, the latter being sometimes a conglomerate of quartz pebbles and silicious sand; in one instance, similar pebbles are contained in a base of dolomite. Beds frequently occur in which the carbonate of lime has been mixed with silicious sand, in some cases yielding an arenaceous limestone, while in others, a chemical union has produced beds of tabular spar, often passing into pyroxene from an admixture of magnesia. These minerals sometimes form beds, in a nearly pure state, but in other cases they are intermixed with quartz, carbonate of lime, orthoclase, scapolite, sphene and other species.

The limestones are sometimes coarsely crystalline, at others finely granular or almost compact; their color is white passing into reddish, bluish, and grayish tints, which are often arranged in bands coincident with the stratification. Some of the dark grey bands, harder than the adjacent white limestone, were found by Mr. Murray to owe their color to very finely disseminated

plumbago, and their hardness to intermingled grains of rounded silicious sand. The limestone is often magnesian, and the manner in which the beds of dolomite are interstratified with the pure limestone, is such as to lead us to suppose that some of the original sedimentary deposits contained the two carbonates, and that the dolomite is not the result of any subsequent process.

The principal mineral species found in these limestones are apatite, serpentine, phlogopite, scapolite, orthoclase, pyroxene, wollastonite, idocrase, garnet, brown tourmaline, chondrodite, spinel, corundum, zircon, sphene and graphite. All of these appear to belong to the stratification, and the chondrodite and graphite especially, are seen running in bands parallel to the bedding. Magnetic iron ore is sometimes found in beds interstratified with the limestone. The apatite which is in general sparingly distributed, is occasionally very abundant in imperfect crystals and irregular crystalline masses, giving to small beds of the limestone the aspect of a conglomerate. Some of the coarsely crystalline varieties of this limestone give a very fetid odor when bruised.

In some parts of this formation, in the rear of the bay of Quinté, the rocks are less altered than in most other places, and here the limestones, although more or less crystalline in texture, afford none of the fine crystallized species elsewhere met with. The foreign ingredients seem to be mechanically intermixed, giving an earthy appearance to the weathered surface of the rock, or are separated in the form of small grains of pyroxene, showing an imperfect metamorphism. For further descriptions of the rocks of this series, see the Reports of the Geological Survey of Canada, particularly that of Mr. Logan for 1846, and Mr. Murray's for 1853; also Dr. Emmons's Report on the Northern District of New York. In position and in lithological characters, the Laurentian series appears to correspond to the old gneiss formation of Lapland, Finland and Scandinavia.

In the second class we include the crystalline limestones of western New England, and their continuation in southeastern New York, and the adjacent parts of New Jersey and Pennsylvania. The limestones of the Champlain division of the Lower Silurian rocks which are found on the Yamaska River, enter Vermont near Misisquoi Bay, where they show a commencement of alteration. Farther south, they become the white granular marbles of western Vermont, and of Berkshire, Massachusetts, which according to Hall, still exhibit upon their weathered surfaces, the fossils of the Trenton limestone; thence passing southwest, they cross the Hudson near West Point, and appear in Orange and Rockland counties, New York, and in Sussex county, New Jersey, in a highly altered condition, closely resembling the crystalline limestones of the Laurentian series, and containing in great abundance the same imbedded minerals. These limestones are some-

times dolomitic, and Hitchcock observes that in the granular marbles of Berkshire, pure and magnesian limestones occasionally form different layers in the same bed. (Geology of Massachusetts, p. 84.)

In Orange county, according to Mather, it is easy to trace the transition from the unaltered blue and gray fossiliferous limestones of the Champlain division, (including the Calciferous sandrock and the Trenton,) to the highly crystalline white limestone with its characteristic minerals. (See his Report on the Geology of the first district of New York, pp. 465 and 486.) This view is fully sustained by H. D. Rogers in his description of the limestones of Sussex Co., given in his final report on New Jersey, (cited by Mather as above, p. 468 et seq.) Mather farther concludes very justly that all the limestones of western Vermont, Massachusetts and Connecticut, and those between the latter state and the Hudson River, are in like manner altered Lower Silurian strata. (p. 464.) From the similarity of mineral characters, he moreover supposes that the crystalline limestones about Lake George are of the same age, and he extends this view to those of St. Lawrence County. Both of these however belong to the Laurentian series, and are distinguished by their want of conformity with the Champlain division, and by their association with labradorite and hypersthene rocks which seem to be wanting in the altered Silurian strata. The slates of this division in Eastern Canada, generally contain some magnesia, with very little lime, and four or five per cent. of alkalies, chiefly potash;* hence the feldspar which has resulted from their metamorphosis is generally orthoclase, and they have yielded gneiss, and mica slate, which with quartz rock, and chloritic and talcose slates, make up the Green Mountains.

In the upper part of the Champlain division, there are found some beds of a limestone, often conglomerate, which is generally magnesian and ferruginous, and often contains a great deal of silicious sand; and associated with it are beds of carbonate of magnesia without a trace of lime, though sometimes very silicious. These beds are interstratified with slates and sandstones, and in the metamorphic region are replaced by the serpentines, which are often intermixed or associated with limestones and dolomites, and, with their accompanying talcose slates, may be traced one hundred and thirty-five miles in Canada, and thence by Vermont, Massachusetts and Connecticut, through New York, New Jersey, Pennsylvania and Maryland, southward. These rocks are everywhere marked by the occurrence of chromic iron ore, in masses running with the stratification, or in disseminated grains, in the serpentine, and sometimes in the dolomite; they are also the

* See my remarks On the Composition and Metamorphoses of some Sedimentary Rocks, L. E. and D. Philos. Magazine for April, 1854, p. 233.

auriferous rocks of the great Appalachian chain. Gold, associated with talcose slates, serpentine, chromic and titaniferous iron ores, is traceable along their outcrop from Canada to Georgia. Gold-bearing veins have also been found in the slates which in Eastern Canada, form the base of the Upper Silurian. I remark that in a somewhat chloritic and very silicious magnesian limestone, which is associated at Granby with red and green slates and sandstones, a portion of oxyd of chromium was detected by analysis. I have also found titanium in some of the very ferruginous slates, which by their alteration become chloritic schists holding magnetic and specular iron, ilmenite and rutile.

Serpentine is found as an imbedded mineral in the Laurentian limestones, but the extensive deposits of serpentine rock, with its associated talcose slates and chromic iron, appear to be confined to the upper part of the altered Champlain division. The examinations of C. U. Shepard, and those subsequent of J. Lawrence Smith and G. J. Brush, have shown that many at least of the so-called serpentine rocks of northern New York, are hydrous silicates of alumina, iron, and potash, containing very little lime or magnesia; they are the dysyntribite of Shepard.

As the northwestern limit of the metamorphic belt in Eastern Canada runs southwesterly into Vermont, the undulations of the strata, which are nearly N. and S., escape from it to the northward. Proceeding E. S. E. however, from the unaltered Trenton limestones of the Yamaska, we cross the overlying slates, sandstones and dolomites, and entering the metamorphic region find the serpentines, talcose, chloritic and micaceous schist, with gneiss and quartzite, very much disturbed, and repeated by undulations. On reaching the valley of Lake Memphramagog, we come upon the third class of crystalline limestones, which are Upper Silurian. This limestone formation has a continuous outcrop from the Connecticut valley, by the lake just mentioned and the upper part of the St. Francis river, to the Chaudière, and is thence traceable by intervals as far as Gaspé, where it is clearly unconformable with the Lower Silurian. It holds the characteristic fossils of the Niagara group, but for some distance from the line of Vermont, is so much altered as to be white and crystalline, and to contain abundance of brownish mica, the fossils being often obliterated. At Dudswell on the St. Francis, the beds of white granular marble show upon their weathered surfaces or in polished sections, the forms of encrinal discs and corals, among which the characteristic *Favosites gothlandica*, and various species of *Porites* and *Cyathophyllum*, have been identified. These fossils in a similar condition are also found at Georgeville on Lake Memphramagog. Following the section in a S. E. direction, to Canaan on the Connecticut river, we meet with calcareo-micaceous schists, which are gradually replaced by mica slates with quartzose beds.

Some of the fine dark-colored mica-slates exhibit crystals of chiastolite, and others near Canaan, abound with black hornblende and small garnets. (For the details of this section see Mr. Logan's Report for 1847–48.)*

These Upper Silurian strata constitute the micaceo-calcareous rocks of Vermont, which Prof. Adams traced through the state, to Halifax on the border of Massachusetts, and they are continued in what Hitchcock has called the micaceous limestones of this state, which according to him pass by insensible degrees into mica slate. The limestones of Coleraine, Ashfield, Deerfield and Whately, Mass., belong to this formation, and perhaps also the crystalline limestone which is found at Bernardston, with magnetic iron and quartz rock, and shows imperfect fossils upon its weathered surfaces. (Hitchcock's Geol. of Mass. p. 560.) The condition of these limestones resembles that of the granular marbles on the other side of the Green Mountains, and they nowhere exhibit that degree of alteration which distinguishes the latter farther south. The same calcareo-micaceous rocks are conspicuous in western Connecticut; but in the towns of Salisbury, Sharon and Canaan the crystalline limestones, and in Litchfield and Winchester, the serpentines, of the Lower Silurian are met with, and these rocks appear again in the southwestern part of the state.

In the fourth class we include the crystalline limestone of eastern Massachusetts, which occurs in a great number of places in the towns of Bolton, Boxborough, Chelmsford, Carlisle, Littleton, Acton, Natick and Sherburne. It appears according to Hitchcock, in interrupted lenticular masses, lying in the gneissoid formation, or in the hornblendic slates, and occasionally presenting distinct marks of stratification. Still farther east at Stoneham and Newbury, we find crystalline limestone, sometimes magnesian, in irregular masses, lying in a rock intermediate between syenite and hornblende slate. Serpentine is found with that of Newbury; and at Lynnfield, a band of serpentine has been traced two or three miles N. E. and S. W. Dr. Hitchcock, to whose report on the Geology of Massachusetts we are indebted for the present details, says of this serpentine, "I am satisfied that it is embraced in the great gneiss formation, whose strata run from N. E. to S. W. across the state." p. 159. He further remarks of the syenite of Newbury and Stoneham, which includes the crystalline limestones, "I have every reason to believe that it is only a portion of a gneiss formation which has undergone fusion to a great degree; for portions of the rock still retain a slaty or stratified structure," and he conceives it probable that all the crystalline limestones of Massachusetts are of sedimentary origin; p. 586. It may be remarked that the irregular shape of these interstratified

* See also on the Geology of Canada, this Journal [2] vol. ix, p. 12, and xiv, p. 224.

masses, is analogous to the interrupted stratification and lenticular beds, frequently met with in fossiliferous limestones.

The limestones of Bolton, Chelmsford and the adjoining towns, are in general highly crystalline, and are remarkable for the variety of fine crystallized minerals which they contain. Among these are apatite, serpentine, amianthus, talc, scapolite, pyroxene, petalite, chondrodite, spinel, cinnamon-stone, sphene and allanite, which include the species characterizing the Laurentian and Lower Silurian metamorphic limestones. The limestone of these quarries evolves a very fetid odor when bruised. Chromic iron ore has never, so far as I am aware, been observed with the serpentines of this region.

We have now to inquire as to the geological age of this great mass of crystalline rocks which is so conspicuous in Eastern New England. Mr. Logan has shown that the rocks of the Devonian System in Gaspé, assuming the Oriskany sandstone as its base, attain a thickness of more than 7000 feet, and as they are still 2500 feet thick in New York, and do not die away before reaching the Mississippi, it is to be expected "that they would follow the Upper Silurian zone in its southwestern course from the eastern extremity of Gaspé, and display a conspicuous figure either in a metamorphic or unaltered condition, between it and the carboniferous areas of Eastern America; to one of which New Brunswick belongs, while another is met with in the state of Rhode Island, and in a metamorphic condition in Massachusetts." (Report for 1848, p. 58.) The lower part of the Devonian, farther west embraces beds of limestone, but in Gaspé the formation consists almost entirely of siliceous and argillaceous beds; in Mr. Logan's section of the whole 7000 feet on the Gulf of the St. Lawrence, he observed only one small bed of limestone, and a few thin bands of limestone conglomerate. When we consider the geographical position of the Upper Silurian rocks in the Connecticut valley on the one hand, and the coal field of southeastern Massachusetts on the other, we can scarcely doubt that the intermediate gneissoid, and hornblendic rocks, with their accompanying limestones, are the Devonian strata in an altered condition. Prof. Agassiz, from his own examination of the region, was led to a similar conclusion as to the age of the so-called syenites, and in August, 1850, presented to the American Association for the Advancement of Science at New Haven, a paper on the Age of the Metamorphic rocks of Eastern Massachusetts, which has never I believe been published. The less altered limestones which, according to Dr. Hitchcock are found interstratified with red slates at Attleborough and Walpole, may correspond to those which with similar slates and sandstone, are met with at the base of the carboniferous formation in Canada on the Bay de Chaleurs, and in New Brunswick.

We have then distinguished four classes of crystalline limestones: first, those of the Laurentian series with their accompanying garnetiferous gneiss, labradorite and hypersthene rocks; secondly, those of the Lower Silurian formation, with their attendant auriferous rocks, talcose slates and chromiferous serpentines; thirdly, those of the Upper Silurian age, with their associated calcareo-micaceous schists; and fourthly, those which belong to the gneissoid rocks of eastern Massachusetts, and are probably of the Devonian period.

I have endeavored in this paper to bring together the facts known with regard to the different crystalline limestones, and their associated strata in this portion of the continent, and to show how far these may serve as a guide in the geological investigation of the metamorphic rocks. While the result confirms the observations of European Geologists, that similar crystallized minerals may occur in the metamorphic limestones of very different geological epochs; it also shows, that within certain limits, the mineral characters of the altered silicious strata, may serve as important guides to our investigations.

Art. XXV.—*Documentary Publications and Science in the Coast Survey Report for* 1853.*

Congressional printing is no longer beneath criticism. It had reached such a depth of degradation in respect to paper, type, proof-reading, press-work, binding and unpunctual delivery, that not even the long suffering of Congress could further endure its vexations and malpractices, however serviceable these might be thought to a party or pet contractor. From Faustus to Little & Brown's last imprint, typography could scarcely show worse specimens than some of the Congressional contract documents, from the Mexican war dispatches to the reorganization of the public printing, about two years since. The chief provisions of the law of reorganization are, one for the election of the same or separate public printers by the Senate and House, the rates and style being carefully defined in the law; one, instituting a responsible superintendent of public printing, appointed by the President, whose business it is to make sure of the proper execution of all printing ordered, in respect to manner, time and quality; and finally, one, directing that a general contract be made for the supply of all the document and bill paper used in public printing, this contract being made by the superintendent, who is also re-

* Report of the Superintendent of the Coast Survey, showing the progress of the Survey during the year 1853. Washington, D.C. Robert Armstrong Public Printer. 1854. Quarto Report, 88 pp. Appendix, 180 pp. Total pages 276 and 54 sketches.

sponsible for its proper fulfillment. The whole efficiency of this system is dependent on the capacity and integrity of the superintendent of public printing, who should be not only an honest man, but a man of administrative capacity and of technical acquaintance with the details of printing, a judge of the quality of paper, a critic of engraving and of the varieties of engraved prints, and a thorough proficient in the printing usages of Congress. This appointment has thus far, we believe, fallen into good hands, and to this, with the praiseworthy seconding of the public printer, we are bound to ascribe most of that conspicuous improvement in all typographical elements so observable in the Congressional documents of the last two sessions. The plan of furnishing inspected contract paper to the printers instead of permitting them to impose on Congress whatever trash they might choose, has proved a capital hit, and will work admirably so long as the contracts are rightly awarded and the contractors held rigidly to the bond. To the recipients of Congressional documents it is so great a blessing to have them decently executed, as certainly they now are in respect to paper and printing, that the recent renovation seems worthy of distinct critical acknowledgment and public congratulation. Unfortunately, such congratulation must stop short of documentary binding, which is still the victim of a wretched system, poorly administered. Indeed, nothing less than a national bindery, a superintendent of binding or the bestowal of the functions of such an office, with a binding assistant, on the printing superintendent, would seem adequate to the cure of existing abuses.

To men of science, Congressional documents are rapidly growing in interest. Much important scientific matter now sees the light in this and only this form. A large portion of the researches, investigations and explorations of the country, are in some wise so related to the general government, as to find their fitting place in the immense series of Executive documents and reports of committees. If to these be added the scientific publications of the State governments, it is really quite surprising to observe how large a portion of the labors of our scientific men are published through these channels. This is doubtless a natural result of the great preponderance of descriptive research and science in a country so unexplored as ours, and in which for that reason, natural history, botany, mineralogy, descriptive geology, geography and meteorology, rightly occupy leading places, and specially enlist governmental patronage. For a time, general and abstract researches will, and legitimately may, give place to the labors of the literal historiographer of nature, though this discrimination ought on no account to survive the occasion for it.

Most of the descriptive science published by Congress has been in connection with the various expedition reports by the

government officers employed from time to time in exploring our western territory, and those from foreign shores, important to our commerce though too little known. Investigations into the botany, natural history, geology, meteorology, topography and agricultural capacities of the various sections explored, have formed integral and essential parts of these explorations, and of course their results have been duly incorporated into the several reports. From Lewis and Clark, Long and Nicollet, down to the present time, these expedition reports have been growing in number, interest and value. The explorations of Wilkes, Fremont, Abeel, Pope, Peck, Cook, Whiting, Miehler, Simpson, Cross, Sitgreaves, Stansbury and Gunnison, Marcy and McClellan, Emory, Whipple, Williamson, Evans, Stevens and McClellan, have been or soon will be formally reported to Congress, and together they constitute a large part of the reliable information now published on our immense western and southwestern territory. In addition to these have been or soon will be published on foreign countries, the Wilkes Exploring Expedition narrative, maps, and scientific descriptive volumes, Lynch's Dead Sea, De Haven's Arctic exploration report, Herndon's and Gibbon's Amazon reports, the reports and results of Gillis's Astronomical expedition to Chili, the reports of the Japan expedition, Ringgold's North Pacific expedition, Page's La Plata exploration, an African exploration, &c. Add to these Foster and Whitney's Reports on mineral lands, Owen's Geological Report, Schoolcraft's Indian publications, the Census Reports, the Patent Office Reports, the Coast Survey Reports, the Smithsonian Reports, and the multitude of less pretending reports on scientific subjects (such as the Capitol extension, building stone experiments, Espy's reports, boiler explosion reports, on anæsthetic agents, &c.) embraced in the file of Executive documents and reports of committees: the resulting aggregate of matter possessing scientific value thus published by Congress, far exceeds our natural anticipation both as to amount and importance.

Unfortunately, the scientific value of materials published in the documentary series, whether of Congress or of State legislatures, is very much impaired by the unsystematic and injudicious plan *of distribution* actually pursued. Men of science to whom particular reports would be of direct practical use, are often entirely unable to procure copies of them, while many men of more political importance, but who will never even look into them, have these same reports profusely lavished upon them. Valuable documents which are reported to applicants as all exhausted, do wholesale duty as wrapping paper for Washington grocers and market men, at a standard price of four cents a pound, maps and plates included. This subject of documentary distribution deserves the serious attention of Congress, and it would not seem

a vain hope that some system could be devised which would be indefinitely superior to that now prevailing, as well in respect to securing rigid responsibility for documents as property, and in promoting the economy, order and convenience of their practical distribution, as in the more important point of securing something like fitness in sending special documents to their appropriate recipients. Distributing Owen's Geological Report to a dry goods importer and the Treasury report on commerce to a geologist, would seem too great an absurdity to exist if we did not know that hundreds of truly valuable volumes are annually thus wasted. This place is not the fitting one for a full discussion of this subject, but it does seem specially appropriate here to state, that a general wish certainly prevails among our scientific men, for the speedy adoption of some system whereby each actual investigator can be regularly and certainly furnished with the exact documents he needs. To purchase these works at regular publishers prices, would be on the whole better for them, despite their notorious brevity of purse, than the present system of lottery distribution; but save in a few exceptional cases, regular purchase is impracticable. The British system of publishing parliamentary documents at moderate fixed prices, would undoubtedly be more acceptable to cultivators of science than the existing chaotic practice which sends away the larger portion of Congressional and State reports on scientific matters. In half the instances, a report when needed is not now obtained at all, either by application or purchase, and when purchased, it is almost always at an exorbitant price. This whole subject deserves consideration and reforming action.

We believe that the printing orders of Congress will enable the Superintendent of the Coast Survey to send copies of his Report for 1853 to the active cultivators of science and such other persons as would find it of real utility, application being duly made to him in Washington, with the name, address, occupation and special scientific or practical pursuits of the applicant. From this Report we will now abstract in a few pages the points of chief scientific interest embraced. Having been favored with the sheets in advance of binding, we are enabled to make this abstract in anticipation of the actual distribution of copies, which probably will not begin until sometime subsequent to the appearance of this article.

The Coast Survey has now reached a very regular rate of annual progress, and its operations during 1853 extended into each of the eleven Coast Survey sections constituting the entire United States coast. The progress of reconnoissance, triangulation, topography and hydrography during the year, has been very satisfactory, being much the same as during the previous year: on

this, there is no occasion for present remarks. In proceeding to give an abstract of science in the Report of 1853, we may advantageously make use of the following heads. 1. *Gulf Stream explorations;* 2. *Tides and tide gauges;* 3. *Longitude operations;* 4. *Geographical positions;* 5. *Map projection tables and notes;* 6. *Publishing records and observations;* 7. *Miscellaneous.* The remaining subject matter of the report lacks purely scientific interest, and could scarcely be abstracted. The details of field and office operations, the examinations of light house sites, the lists of parties, &c. are given with the customary fullness, constituting a thoroughly digested record of the year's operations.

Gulf Stream exploration.—This great and singular peculiarity, embracing in its mighty sweep our entire Atlantic offshore vicinage, is so important to navigation and so essential a feature of our coast hydrography, both in its practical and scientific character, that its thorough exploration ought certainly to form an integral part of the Coast Survey, whence our offshore charts are all to be derived. A specific and complete delineation and theory of this unique oceanic movement can only be reached as a result of elaborate and continued observations on all its physical and phenominal elements. This giant problem is thrown down as a gage at our national door, and the honor code of philosophic chivalry bids us accept the challenge. With a clear perception of the requirements of this great research, Prof. Bache in 1845 organized and began the execution of a plan of operations, which provided for running a system of perpendicular sections across the axis of the stream from selected points of the coast and observing at frequent stations along these sections, the several elements required. Between 1845 and 1848, sections were run from Montauk Point, Sandy Hook, Cape Henlopen, Cape Henry and Cape Hatteras; when from accidents and other hindrances, the work was intermitted until in 1853, when sections were run from Cape Hatteras, Cape Fear, Charleston, St. Simons, St. Augustine and Cape Canaveral. The results for 1853 are given in a sketch of detailed sections, and a general delineation of the Gulf Stream in its several component bands or threads, as thus far determined, will be found among the sketches. Over six pages of the Report are devoted to a full exposition of the results already reached.

The element of temperature, superficial and at various depths, has been chiefly observed, up to this time; the instruments used being Six's registering thermometer for moderate depths and Saxton's metallic deep-sea thermometer for the greater depths, a temperature sounding of 2160 fathoms having been made. One general result of the investigation is that "there are alternations

of temperature across the Gulf Stream, cold water intruding and dividing the warm, making thus alternate streaks or streams of warm and cold water. In fact, the Gulf Stream is merely one of a number of bands of warm water separated by cold water." A "cold wall" limiting the Gulf Stream on the shore side, is clearly made out, as also its slight shoreward slope from the warm water overlying the cold. A distinct current of underlying cold water from the northern regions is found alike in the northern and southern sections. "It can hardly be doubted that this cold water off our southern coast may be rendered practically useful by the ingenuity of our countrymen. The bottom of the sea fourteen miles E. N. E. from Cape Florida, 450 fathoms in depth was in June, 1853, at the temperature of 49° Farenheit, while the air was 81° Farenheit. A temperature of 38° (only six degrees above the freezing point of fresh water) was found at 1050 fathoms in depth about 80 miles east of Cape Canaveral. The mean temperature of the air at St. Augustine is 69°·9 Farenheit, and for the three 57°·5. The importance of the facts above stated in reference to the natural history of the ocean in these regions, is very great, but, of course, requires to be studied in connection with other physical data. It has also a bearing upon the important problems of the tides of the coast. This exploration of the Gulf Stream will be steadily prosecuted to its close, the different problems being taken up in turn or in connexion as may be found practicable."

The most remarkable fact brought to light in relation to the Gulf Stream is probably that of the existence of two submarine ranges of hills near its origin, which produced most marked effects on the distribution of its parts. "The form of the Charleston and Canaveral sections," as shown in the diagram, shoals "gradually from the shore to 53 and 36 miles respectively, then suddenly falling off to below the depth of 600 fathoms. On the Charleston section, 96 miles from the coast is a range of hills steep on the land side and having a height of 1800 feet and a base of about eleven miles on the seaward side; a second range 136 miles from the coast, 1500 feet high, with a base of about seventeen miles, on the outer side. Beyond this there is a more gradual rise. On the Canaveral section the inner range is 68 miles from the coast." The effect of this form of the bottom in forcing up the deep cold water stratum is very marked, so that the deep isothermals of section, exhibited a general conformity to the bottom curve. It is undoubtedly due in a considerable degree to these submarine hill-ranges, and to their uplifting of the cold water, that the Gulf Stream is divided into several superficial bands, though to what exact extent and how far subject to variations remain to be studied. Horizontally, the conformity of the Gulf Stream to the coast line configuration is verified even in detail,

and its modifications by the variation of steepness in the off-shore bottom slope, are strongly marked. With these results, the names of Lieuts. Davis, George M. Bache, Richard Bache, S. P. Lee, Maffitt and Craven are conspicuously associated; George M. Bache being distinguished as a martyr to his zeal, in the very glow of talent, hope and success.

The results of the microscopic examinations of seventeen Gulf Stream bottoms made by Assist. L. F. Pourtales (Appendix No. 30), are of great interest. From these and many other investigations of bottoms, he has derived the generalization that the per-centage of shells, chiefly Foraminiferæ, progressively increases with the depth, and he remarks of a bottom from the depth of 1050 fathoms that it "is no longer sand containing Foraminiferæ, but foraminiferæ containing little or no sand. The grains of sand have to be searched for carefully under the microscope, to be noticed at all." It will be seen that this result coincides with Prof. Bailey's recent announcement, thus closely linking the Gulf Stream bottoms with those of the remoter parts of the Atlantic. Mr. Pourtales also somewhat examines the question whether these minute animals lived where they were found, or have been gradually washed down from the reefs. Though not decisive the evidence inclines him to the opinion that they lived where found. This is indicated by the fact that most of the individuals are found perfect, notwithstanding the extreme delicacy of the shells, and again by the delicate pink color of the Globigerinæ, which could scarcely survive transportation. The fact of the occurrence of the same species off the New Jersey coast and off Cuba and other West India islands under very dissimilar circumstances of light and temperature is also indicative that they are actually drawn from their true habitat in these Gulf Stream soundings. Mr. Pourtales well remarks on the importance of "a knowledge of the habitation and distribution of the Foraminiferæ" to geologists, "since of all classes of the animal kingdom, none has contributed so large a share to the formation of rocks, at least in the cretaceous and tertiary formations."

Tides and tide gauges.—It is an indispensable step in the survey of each harbor, river, bay, &c., of the coast, to make special observations on the tides; at least so far as to establish the place of reference to which the soundings shall be reduced and to have adequate tide records for effecting this reduction of each sounding. A tide table with the corrected establishment and notes descriptive of the tidal movements are parts of the engraved matter required to go on each finished chart of the Coast Survey. In the regular prosecution of this work, there thus results a great accumulation of tidal observations which require reduction and discussion before the charts can be completed. Also several permanent tide stations are established along the coast, to furnish by

their minute and continuous records the elements of wider and more critical investigations into tidal phenomena. All these observations are now regularly reduced by a special "tidal party" under the particular direction of Prof. Bache.

The Report of 1853 (Appx. No. 26) contains a very valuable table, embodying the principal reduced results at 64 important tide stations on the Atlantic, Gulf and Pacific coasts. Appendix Nos. 27, 28 and 29, contain elaborate discussions by Prof. Bache, of the tides at Key West, and Rincon Point, San Francisco, in which they are reduced and resolved into results of the physical tidal theory. The curves of the phenomena of the theoretical components are presented in three plates. Prof. Bache thus sums up the tidal peculiarities of our entire coast. (p. 7, Report.)

"It is an interesting fact that the tides of our Atlantic coast, of parts of the Gulf of Mexico, and of the Western coast, are of three different types. Those of the Atlantic coast are of the ordinary type of tides—twice in twenty-four hours—having, however, a distinct though small difference in the height and time between the morning and afternoon tides, known as the diurnal inequality. The Gulf tides are single-day tides, and, until the Coast Survey developments established the contrary, were believed to depend upon the winds which have the character of trade-winds, and, therefore, considerable regularity along that coast. The tides of our Pacific coast ebb and flow twice in twenty-four hours, but with so large a diurnal irregularity in height that the plane of reference of mean low water, commonly used on the charts, would if employed be a snare to navigators. A rock in San Francisco bay, which at one low water of the day might be covered to the depth of three and a half feet, might at the next be awash. The observation of the tides on the Atlantic coast having been made in close connection with the other parts of the hydrography, the stations still wanting will be filled up as we advance. A few stations are still required on the Gulf of Mexico to complete the general determination of its tides from Cape Florida to the Rio Grande. We have already found nearly the dividing position, Cape St. George, Apalachicola, where the tides resemble on the one side, eastward, those of Cedar Keys, Key West and Tampa Bay, ebbing and flowing twice each day, with a large diurnal inequality, and on the other, westward, resemble the tides at Mobile entrance, the Delta of the Mississippi, Galveston and the Rio Grande entrance, ebbing and flowing, as a general rule, but once in twenty-four hours."

The Report contains a detailed description of Saxton's self-registering tide gauge (Appendix No 38. Sketch No. 54); a report of operations in establishing a tide gauge with a pipe leading seaward on a difficult open coast near Nantucket (Appendix No. 13); and finally a report of operations in obtaining off-shore or

open ocean tidal observations, on a shoal a mile and a half from land. (Appendix No. 15.) The excellent results from Saxton's gauge lead to high expectations from the records now regularly received from three permanent and three movable Saxton gauges, operating on our Western coast. The importance of separating the true tide wave from the heaping up of water along shore, leads us to watch with peculiar interest the off-shore observations and to hope for their success at much greater distance from land.

Longitude operations.—It is now esteemed essential where practicable, in conducting the survey, to refer at least one principal station in each section, to the central longitude point (Seaton station, Washington), by a telegraphic determination of longitude differences. During the year 1853, operations were conducted for thus connecting Charleston with Seaton station, the longitude difference already found by Mr. Walker in 1850, being only a preliminary determination. Such was the imperfect condition of insulation of the telegraph wires, as found by repeated trials, that it became indispensable to establish an intermediate station and Raleigh was thus occupied. Dr. B. A. Gould's report of these operations is given in Appendix No. 33. Some observations were also made on the velocity of the galvanic wave, and the personal equations of the observers were duly compared. Charleston will soon be in turn similarly connected with New Orleans.

Prof. B. Peirce reports (Appendix, No. 31) the results of his investigations and of some observations made under his charge, for the purpose of ascertaining a method of determining, "the longitude from observed transits of the moon, which shall not be involved in the great and singular errors of the lunar ephemeris." After stating the faults of the present methods, in which standard corresponding observations of moon culminations are interpolated, he concludes that as the existing lunar theory will not stand the test of observation, a correct ephemeris is not now practicable. He then, from three hundred and sixty-seven special comparisons, determines the standard probable error of an observation of a lunar transit as one-tenth of a second of time. An attempt to determine an annual empirical correction for the lunar elements failed, and gave place to the determination of a constant error of epoch and a periodical error running through a half lunation. For Greenwich observations, 1847, the probable error of ephemeris longitudes thus corrected, when compared with observation, came out very nearly the standard probable error of a transit observation. This investigation has been since continued.

Prof. W. C. Bond reports (Appendix, No. 32) the results of some operations for testing the accuracy of the spring governor records. Numerous star transits of Spica (110 are given) over the wires of the Cambridge equatorial were simultaneously recorded by two spring governors, differing one-tenth of a second

in their pendulum-vibration times: one governor was at Cambridge and the other at Haverford, Penn. The result of scrupulous comparisons of their records, indicates no discrepancy exceeding three hundredths of a second ascribable to the imperfect equalizing action of the spring attachment.

Mr. G. P. Bond has reported in considerable detail, (Appendix No. 34,) on the computations of the chronometric expeditions of 1849, '50 and '51, for determining the difference of longitude between Cambridge, Mass., and Liverpool. He details the precautions taken to ensure accurate reductions of the transit observations for local time and for evolving errors of observation. He discusses the micrometric and level division values, the azimuth and collimation errors, lateral refraction, personal equations, clock errors, the position of the midwire of the transit, the pivot figures, the errors of comparing the chronometers with the standard clock, and the irregularities of chronometer and clock rates. The general results of the computations have since been submitted to the American Association at the Washington meeting, when Prof. Peirce announced additional discussions of moon culmination longitude methods, in reference to the longitude of America. We are now near the final fixation of the standard longitude difference between our system of connected stations and that of Europe: which difference once authoritatively established, will doubtless be liable to no future change, unless by submarine telegraphic determinations.

Geographical Positions.—In the Coast Survey Report for 1851, is a list of 3240 stations, to which an addition of 600 is made in the Report of 1853, (Appendix No. 7.) For each of these 3840 stations, a latitude and longitude is given. Also for each of the lines connecting these 3840 stations, as shown in the section triangulation sketches, the length of line is given in metres, yards and miles, and its azimuth in both directions is introduced. This extensive series of geographical positions and of triangulation elements is the product of an immense labor of observation and computation, being indeed the great trigonometrical consummation of the survey up to the present time. It will prove of wide and permanent use to have easily accessible so extensive a series not only of latitudes and longitudes of stations but of accurate distances and bearings between so many intervisible points, along our entire seaboard. In numerous instances, surveyors can conveniently test their compass variations by observing on one of these lines of given azimuths. The enduring value of this list for plotting surveys and maps is self-evident and will not be impaired by the slight corrections to which some of the positions and distances will in future be liable.

The development of *station errors*, or distinct discrepancies between the geodetic and astronomical latitudes and longitudes

of particular stations, is a constantly recurring result of the survey. They are caused by local irregularities in figure and density of the earth and amount in several instances to about three seconds, while at a station of the Ordnance Survey, the station error is nine seconds. The attraction of mountains is not the usual cause of these errors, though a displacement of the vertical to a much greater amount has in some cases been traced to this origin. When it is known that even now, before the mutual verification of sections by connecting their base lines, the tabular distances given in this list are generally considered as liable only to an average error of about one foot in six miles, it will scarcely seem wonderful that the station errors are found to be as distinctly indicated by a comparison of the azimuth and back azimuth observations, as by those for latitudes and longitudes; so that the two results even verify each other quantitatively. The notes introducing the list give a clear insight into its mode of construction and arrangement.

Map projection tables and notes.—The most voluminous adjunct to this report is Appendix No. 39, giving tables for projecting maps with notes on map projections. It is much to be hoped that this valuable accession to the means of accurate map construction will accomplish something towards effecting a reform in the very imperfect chartographic practice, now too widely prevalent.

The notes present in a condensed form a classified synopsis of the various projections which have been used. The four classes into which these are distributed are based on their peculiar modes of mathematical genesis. The distinctive features of eighteen species of projections, are briefly and systematically presented. Bonne's projection, being that chiefly used in Europe for topographical surveys of considerable areas, is discussed in greater detail. Still more space is given to the *polyconic* projection, which is that used in the Coast Survey office. This name is new, and the two varieties, called *rectangular* and *equidistant* are both in use and require the same tables. Fortunately these methods can now be employed by any intelligent draftsman, furnished with this report, in constructing any local, county, state or general map, within the United States. Full instructions are given under a special head for the graphic construction of the rectangular and equidistant polyconic projections. The formulæ used for computing the tables, also the constants employed and their logarithms are given, though without the detailed derivation of the formulæ.

The Tables are six in number. Table I. gives the relation between the units of length used in different countries—Table II. has for its object to facilitate the conversion into each other of metres, yards and statute miles, and will be found highly con-

venient in many computations—Table III. gives the length, in statute and nautical miles, of a degree of the meridian for each 5° between latitudes 20° and 50°—Table IV. gives the length of a longitude degree for each degree parallel, between Latitudes 17° and 50°, expressed in nautical and statute miles and meters —Table V. gives the lengths of the parallel and meridian arcs and coördinates for projecting large maps in the United States and can be used for a map embracing the area between Latitudes 17° and 50° and extending 70° in Longitude, which limits include considerably more than the entire United States. Table VI. gives the lengths of the arcs of parallels to seconds for each minute of Latitude between 24° and 50°; it also gives the meridian arcs and coördinates with corresponding accuracy. This table is available for constructing any local map projection on a large scale, anywhere within the latitude specified. For state or general maps, Table V. should be used, and for town and country maps, &c., Table VI. is required. It will be seen that these tables suffice for all the geographer's needs within our national limits, while a little study and practice will enable any one to use them correctly and rapidly. The superiority of the projection on which these tables are based, should induce its general use for all the purposes indicated.

Publishing Coast Survey records and observations.—We observe with satisfaction that the estimate for the ensuing year, embraces an item of $20,000 for the long desired and oft recommended publication of records and observations, made in the progress of the survey. It is earnestly to be hoped that the experience of the British Ordnance Survey in relation to this class of publications will not be lost on us. Why should we wait until their day of greatest usefulness is past, until all freshness of interest has departed, before doing what must be done at last, and what ought for all positive reasons to be done now? Delay is most thriftless policy in this case, and especially as the Treasury is now plethoric with surplus revenue. The whole matter is thus clearly set forth by the Superintendent. "The history of such works shows that the observations accumulated during their progress and which must be published for permanent reference and to give them authenticity, are brought out very slowly. Those who have taken part in them are dispersed, and questions arise which require their aid to answer. However perfectly in theory a work is organized, such questions will arise. The interest in the results is lost with the responsibility for their accuracy. The present time, when the organization is complete, and the observers are still connected with the work, is the proper time, on every account to publish the observations. The economy of present publication would be very considerable. I am sustained in these views by the judgment of the scientific men of

the country generally." We certainly hope to see this work soon commenced.

MISCELLANEOUS.—Among the operations in Maine are reported some measurements of heights by nearly all practicable modes. From these and other operations there, we see evidently looming forth "important data for the coefficient of refraction under different circumstances, and in relation to the relative advantages in accuracy, time and other particulars of the different modes of measuring heights."—At East Base near Galveston, an elaborate set of latitude and magnetic observations is reported.—The list of Coast Survey nautical discoveries and developments for the year embraces nineteen items, chiefly of shoals, rocks, banks and changes in bars, inlets and harbors. The Gulf Stream submarine hill-ranges, before mentioned, are the crowning discovery.—The 54 Sketches appended to the Report, embrace much new hydrography of importance to navigation, and among other subjects of congratulation is specially noticed, the completion of the troublesome but highly important hydrography of Nantucket Shoals.—Appendix No. 36, consists of notes on lithography and lithographic transfers. The sketches give evidence of the value of the transfer process, being all (23,000 copies from each of 54 plates) printed by its aid, without serious wear of the copper plates. The subject of adapting engraving to transfer printing is touched upon and is of much importance to such as are about using the transfer process.—Appendix No. 37, describes a novel instrument called the interranger, for enabling boats conveniently to run on range lines between opposite stations; also some account of various devices tried.—Appendix No. 35, gives the results of two analyses of deposits taken from the boiler of the steamer Hetzel. This is a subject of practical importance and it is to be hoped that some corrective may grow out of such analyses.—Appendix No. 43 will be interesting to those who feel how great a loss the country sustained in the death of Sears C. Walker.

There is much more in the various field and office operations, which might interest scientific readers, but space bids us refrain. In conclusion, we may remark, that taken as a whole, this report equals or exceeds any of its predecessors in the extent and value of its contributions to science; and that by its paper, typography, indexes and sketches, it goes far towards reasserting the admissability of a Congressional Document to respectable libraries. H.

ART. XXVI.—*On the use of Hydrogen Gas and Carbonic Acid Gas, to displace Sulphuretted Hydrogen in the analysis of Mineral Waters, &c.;* by Prof. W. B. ROGERS and Prof. R. E. ROGERS.

First.—On the use of *Hydrogen Gas* in the analysis of Sulphureous Waters.

One of the most difficult points in the analysis of mineral waters is the determination of the sulphur which is contained in many of them in the two conditions of Sulphuretted Hydrogen, and a sulphid, either of an alkaline metal or of magnesium or calcium. No satisfactory process has we believe yet been devised for this purpose. It is easy enough by the nitrate of silver or chlorid of copper to determine the total quantity of sulphur present in these compounds; but in the subsequent process of boiling the liquid preparatory to the precipitation of the sulphur of the sulphids, while we expel the free hydrosulphuric acid, we at the same time decompose the sulphid of magnesium or calcium which may be present, even when the process is conducted out of contact with the air, as in an atmosphere of hydrogen gas; and if we boil the liquid in the air or even expose it for some time to the atmosphere at common temperatures, the sulphids of sodium and potassium as well as of magnesium and calcium, are gradually decomposed by the carbonic acid of the air evolving their sulphur in the condition of hydrosulphuric acid. At the same time by the action of the atmospheric oxygen a portion of the alkaline sulphid is converted into hyposulphite. It is therefore desirable to discover some method of separating the free hydrosulphuric acid without at the same time affecting the other sulphur compounds present in the water. This we think we have attained by transmitting through the sulphureous water a stream of hydrogen gas. In repeated trials made with natural hepatic waters, among them the celebrated Blue Lick water of Kentucky, and with an aqueous solution of hydrosulphuric acid, prepared for the purpose, we have found that by continuing for a sufficient time the washing action of the hydrogen, we could reduce the sulphuretted hydrogen to an almost insensible trace. A volume of 25 cubic inches of the Blue Lick water submitted to this action for one hour retained only a minute fraction of the original charge, and in one and a half hours it gave only the slightest appreciable trace of sulphuretted hydrogen.

The hydrogen used for this purpose, before reaching the vessel which contains the mineral water, is conducted through a solution of potassa in order to remove any hydrosulphuric or carbonic acid it may contain. Thence it is made to pass into a second

vessel containing the sulphureous water through which it bubbles in a brisk but not violent stream. The gas, more or less charged with sulphuretted hydrogen, is led into a third vessel containing either a solution of nitrate of silver to which ammonia has been added, or an alkaline solution of arsenious acid, to arrest the sulphuretted hydrogen. The former solution is greatly to be preferred where the mineral water is only feebly sulphureous. The sulphur thus precipitated is to be determined in the usual way.

Suppose the mineral water to contain free hydrosulphuric acid together with sulphids say of potassium and magnesium, we may proceed as follows:

1. We determine for a given volume of the water the total amount of sulphur present by the use of chlorid of copper or nitrate of silver.

2. We subject an equal volume of the water to the hydrogen current until the escaping gas gives only the faintest trace of hydrosulphuric acid when the jet is received on a surface of white porcelain rendered moist by a mixture of nitrate of silver and ammonia. The mixed gas being passed into the silver or arsenious solution, gives a precipitate from which we determine the amount of free hydrosulphuric acid in the water.

3. We apply heat to the flask containing the sulphureous water which has been thus treated, so as to cause gentle boiling, at the same time supplying the upper space with hydrogen in a moderate but steady stream. It will be found that below the point of ebullition the issuing hydrogen will give scarcely a trace of hydrosulphuric acid, but as soon as the liquid begins to boil, the stream of vapor and hydrogen plainly shows the presence of this substance, then slowly evolved by the decomposition of the sulphid of magnesium or calcium.

4. We treat the remaining liquid with chlorid of copper, or the arsenious solution, to determine the sulphur of the alkaline sulphid which is the only sulphur compound left in the water. The sum of this and the sulphur of the free hydrosulphuric acid subtracted from the total quantity of sulphur gives that of the sulphid of magnesium.

We find that a proportion of hydrosulphuric acid too small to be quantitatively determined by precipitation from the water itself can be ascertained by the use of the stream of hydrogen. It is only necessary to pass the gas which has been transmitted through the water into an ammoniacal solution of nitrate of silver in a long test tube or Liebig's bulb. By continuing the action for one or two hours we obtain a precipitate capable of being separated.

When the water contains no sulphid of magnesium or calcium, it is merely necessary, after determining the total amount of sulphur present, to boil the liquid in an atmosphere of hydrogen

as long as the gas gives a distinct trace of S H by the tache on porcelain as before described; and then by precipitation to determine the quantity of sulphur in the alkaline sulphid of the remaining liquid.

From what has been said it is obvious that the only practical objection to the process here proposed is the tardiness of the displacing action of the hydrogen gas; but considering the acknowledged imperfection of the methods in use, we think that it may be found worthy of adoption.

Second.—On the use of *Carbonic Acid Gas* in the analysis of mineral waters containing Sulphuretted Hydrogen.

As might be inferred from its great absorbability by water, carbonic acid acts much more rapidly than hydrogen in separating hydrosulphuric acid from that liquid. To assure ourselves of this effect, we made several experiments with natural and artificial sulphureous waters, all of which led to the same result. The following example will suffice to show the efficiency and promptness of the displacing action of the carbonic acid.

Twenty-five cubic inches of Blue Lick water contained in a narrow necked bottle, were subjected to the washing action of a brisk stream of carbonic acid gas previously purified by transmission through water. In fifteen minutes the liquid, tested by ammoniacal nitrate of silver, gave a scarcely discernible trace of hydrosulphuric acid, and in twenty minutes not a vestige of it could be detected by the same reagent. The rapidity and completeness of the separation are as striking as the ease with which the experiment can be made.

When therefore a mineral water is known to contain sulphuretted hydrogen only in the free state, we would recommend as the simplest and most exact method for determining this ingredient, to pass through the liquid a stream of washed carbonic acid gas, and to arrest the hydrosulphuric acid, by conducting the current of mixed gas into an ammoniacal solution of nitrate of silver in a small flask or Liebig tube. The precipitated sulphuret being mingled with only a small volume of liquid, admits of more easy separation and determination than when formed in the usual way by adding a precipitant to a large mass of the mineral water. In the case of feebly sulphureous waters this method is we think greatly superior in accuracy as well as promptness to any of those in use. By operating on a considerable volume of the water, the flask or tube will furnish the precipitated sulphuret in sufficient amount for a quantitative determination in cases where in the ordinary way no separable precipitate would be obtained.

As carbonic acid is capable of decomposing the sulphids contained in a mineral water giving rise to free hydrosulphuric acid,

it cannot be employed for determining the quantity of the latter when associated in the water with a sulphid. In this case the stream of carbonic acid would carry with it the hydrosulphuric acid due to its reaction with the sulphids, as well as that existing ready formed in the liquid. For such a water, hydrogen gas used as above explained, is the proper displacing agent.

ART. XXVII.—*On Changes of the Sea-Level effected by existing Physical Causes during stated periods of time;* by ALFRED TYLOR, F.G.S.

(Concluded from page 32.)

PART II.

ALLUSIONS have already been made to the difficulty of proving whether or not the sea-level had been *gradually* elevated, because the rise of the waters would conceal the evidence of their former height except just at the mouths of rivers, where deposits of fluviatile alluvium might raise the land from time to time and keep it above the waves. The recent strata formed at a few such localities have been described by the best observers; and while there are appearances in several cases which might be to some extent explained by the supposition of a gradual rise of the sea-level, yet no proof could be obtained without the concurrent testimony of a much greater number of instances than have yet been brought forward. Sufficient information, it appears, exists to show that the quantity of alluvium in the deltas of such rivers as the Mississippi, Ganges and Po, is so enormous, that the accumulation must have occupied a period of time during which it would not be possible to conceive the sea-level stationary.

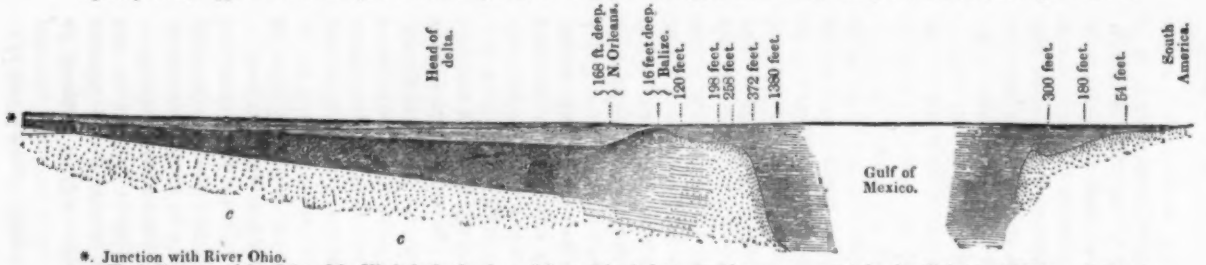
Little progress could be made in an inquiry of this kind without clear views of the operations of rivers. The recent reports of engineers upon this subject supply an important link in the chain of evidence, and enable us to understand the laws which govern the formation of alluvial plains along the lower parts of all river-courses.

The diagram (fig. 8) represents a section of 600 miles of North America, through the alluvial plains and delta of the Mississippi,* together with a section of the Gulf of Mexico, from a point 100 miles east of the Balize to the continent of South America. The sea-bottom is marked from the soundings on the Admiralty Chart, and the depth of the Mississippi and its fluviatile deposit are inserted from statistics collected by Sir C. Lyell.†

* For a most valuable detailed description of the physical geography, &c. of the Mississippi and Ohio valley, see Mr. C. Ellet's paper, Smithsonian Contributions, vol. ii, 1851.

† See note, page 26.

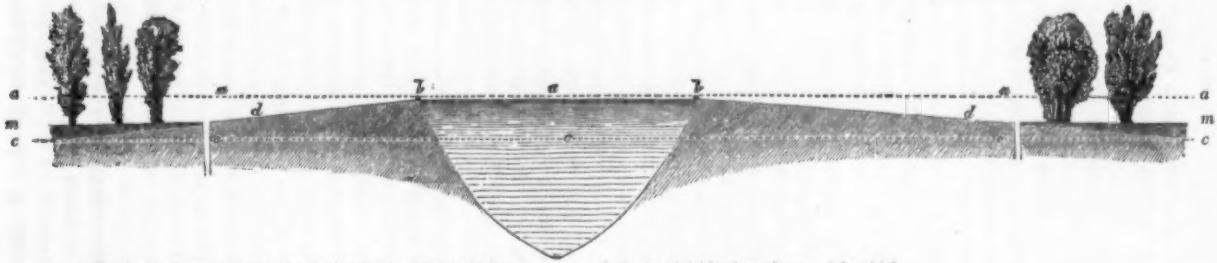
Fig. 8.—*Diagram showing depth of the Delta (supposed, 600 feet); area 14,000 square miles; height of the river above the sea-level 275 feet at *; depth of river, supposed 80 to 200 feet in this diagram; ditto of plains, supposed to average 264 feet; area, 16,000 square miles.*



*. Junction with River Ohio.
a, a. Fluviatile strata of the plains of the Mississippi; the slope of these plains is determined by measurement to be about 1 foot in 10,000 towards the sea.
c. Marine strata.

Direct distances:—Junction with Ohio to Balize, 580 miles. Head of Delta to Balize, 180 miles. New Orleans to Balize, 70 miles.
[Vertical scale 1 inch to 1000 feet. Horizontal scale 1 inch to 150 miles.]

Fig. 9.—*Transverse section of the Mississippi, where it is 1500 feet wide and 100 feet deep, running in the midst of an alluvial plain 50 miles wide. (This diagram shows the section of slow-flowing rivers in general.)* Vertical scale 100 feet to the inch.



a, a. The level of water in the river during flood, which is 25 feet above the level of the distant marshes, *m, m.*
c, c. The level of water in the dry season.
b, b. Artificial banks or levees, 4 feet high.
d, d. The banks and plains.
m, m. Marshes, supplied with water by filtration from the river at all seasons of the year.

The whole body of water in the river must be in motion, so that even in flood time only a small per-centage of the water and alluvium in the stream can escape over the banks.

It will be seen that the level of the water in the Mississippi, near its junction with the Ohio, nearly 600 miles from the Gulf of Mexico, is 275 feet above that of the sea. The slope of the alluvial plains through which the river winds will therefore be less than 1 foot in 10,000.

The hills bordering the valley of the Mississippi are cut through in several places by the river, thereby exposing good sections of their component strata, consisting of alluvial deposits thought to be much more ancient than those we are about to consider.

An area of 16,000 square miles is occupied by the more modern alluvial formation between the head of the delta and the junction of the Ohio.* It is supposed to be, in the average, 264 feet deep, and is from 30 to 80 miles wide. The true delta extends over 14,000 square miles, occupying a frontage of 2¼ degrees on the coast-line of the Gulf of Mexico, and extends 180 miles inland. At its southern extremity its surface is hardly above the level of high tides, but it rises gradually as it passes inland, and at New Orleans is nearly 10 feet above the sea-level.

A boring near Lake Pontchartain, of 600 feet, failed to penetrate the modern alluvium; and wherever excavations are made, the remains of trees are frequently found, apparently in the places where they grew, but now far below the sea-level. Sir Charles Lyell computes its average depth at 528 feet, and consequently nearly the whole of this modern deposit is below the sea-level, yet is supposed not to contain marine remains. The fall of the Mississippi during a course of 600 miles is shown by fig. 8; the depth of the channel varies from 80 to 200 feet until it approaches the Balize, where it shallows to 16 feet. The rise of the tide at this point is only 2 feet. The depth of the alluvial deposit below the river-channel is also indicated, together with the surface of the more ancient formation upon which the Mississippi has formed this great alluvial deposit, the bottom of which is now more than 500 feet below the present sea-level.

Mr. Charles Ellet, Jun., in a Report to the American Secretary of War, January 29, 1851, communicates the information from which the diagrams figs. 1 and 2 are constructed. See p. 23.

The theory of Mr. C. Ellet is, that the velocity of the stratum of fresh water (fig. 1) is communicated entirely to the underlying stratum, composed of salt water, partially to the next stratum 3, but not at all to stratum 4, which is stationary: stratum 5 is also marine, but it flows in an opposite direction to the rest, and restores the salt water which is carried away by the friction of the upper stratum, No. 1, against the suface of No. 2.

It is supposed that the rapid increase of deposit at the bar, fig. 1, arises from stratum No. 5 carrying mud to that point, where its

* Lyell's Second Visit to the United States, 1849, vol. ii, pp. 146–152, 155, 169, 194, 195, 203, 243, &c.

velocity is partially neutralized by impinging against stratum No. 1.

From the following particulars of the deltas of the Ganges and Po, it would appear that they are similarly situated to the Mississippi. "An Artesian well at Fort William near Calcutta, in the year 1835, displayed at a depth of 50 feet a deposit of peat with a red-colored wood similar to that now living. At 120 feet clay and sand with pebbles were met with. At the depth of 350 feet a freshwater tortoise and part of the humerus of a ruminant were found. At 380 feet, clay with lacustrine shells was incumbent upon what appeared to be another dirt-bed or stratum of decayed wood. At 400 feet they reached sand and shingle."*

In the delta of the Po, a well bored 400 feet failed to penetrate the modern alluvial deposit; very near the bottom it pierced beds of peat, similar to those now forming. The coarser particles of mud which have already passed the mouths of rivers may contribute to the marine or fluvio-marine deposits forming outside deltas; but this can only be to a limited extent, as the great bulk of the mud is far too fine to settle near the coast. Little material could be obtained from cliffs along the sea coasts, but we have information of marine currents specially bringing sand and mud from other parts of the sea-bottom to the neighborhood of deltas. (See Mr. Ellet's observations.)

For these reasons, if the further examination of the deltas of the Mississippi and other rivers should lead to the discovery of some recent marine or fluvio-marine strata, it may turn out that such deposits have been more rapidly accumulated than the purely fluviatile beds with which they may be associated. In estimating the age of deltas, allowance, however, ought to be made for such contingences, and also for their organic contents.

Let us now turn to fig. 9, which exhibits Sir Charles Lyell's transverse section of the channel and plains of the Mississippi, and at all points throughout a course of several hundred miles. The dotted lines are introduced to show the variation of the water-level in the wet and dry seasons: *b*, *b* represent the artificial levée; *d*, *d* the banks and plains; *m*, *m* the swamps of the Mississippi. "The banks† are higher than the bottom of the swamps, because, when the river overflows, the coarser part of the sediment is deposited on the banks, where the speed of the current is first checked" (Lyell). The channel, however, is so wide and deep, that even if there were no artificial banks to prevent floods, the river would carry into the Gulf of Mexico the principal mass of the mud it had received with the water of its tributaries; for it is only for a short time in the year that the level of water in

* Lyell, *loc. cit.* p. 248; and Principles, p. 267–270.

† There is a similar section of the Nile and its banks published in the fourth volume of the Quarterly Journal of the Geological Society, p. 344, but communicated by Lieut. Newbold in 1842.

the river is above that of the adjoining plains. The swamps and the numerous lakes formed by deserted river-beuds communicate at all times of the year with the main stream. In these places mud could be constantly deposited mingled with the remains of the vegetation which grows luxuriantly in the swamps. The only supply of inorganic matter for raising the level of the vast plains through which the river winds for hundreds of miles, must be the mud deposited upon them during the periodical floods. These are very much prevented by the artificial levée; but when they do occur, their force is augmented by the water being artificially dammed up.

"I have seen, says an eye-witness, when the banks of the Mississippi burst, the water rush through at the rate of ten miles an hour, sucking in flat boats and carrying them over a watery waste into a dense swamp forest" (Lyell). It would appear that the Mississippi differs in size and proportion more than in other respects from our rivers. For instance, when floods occur upon our own alluvial plains, they are most conspicuous at a distance from the stream which caused them, indicating that the parts of the plains nearest the banks are higher than those at a distance from it, and therefore that fig. 9 would also represent the *transverse section of slow rivers generally*. The similarity of the physical features presented by the lower parts of all rivers was particularly remarked by Hutton.*

It has been observed by engineers,† that in all rivers in this country the large quantities of silt brought into them by winter freshets do not tend to choke the channels, but that, at that period of the year, former accumulations of deposit are actually removed by the force of the stream; and therefore, that although winter-freshets bring down silt with them, they carry into the sea a larger quantity than they have introduced into river channels.‡ If it were allowable to assume that the unequal supply of water at different seasons of the year produces effects in the channel of the Mississippi similar to these just described on our own streams, the following consequences might be deduced from the fact that winter freshets remove more detritus than they bring down. The diminution of the speed of the current of rivers assists the deposition of silt upon their beds, as much as its increased speed in the winter season favors its removal. The summer deposit, however thin it may be, cannot occur without contracting the sizes of the channel.

* Theory of the Earth, vol. ii, p. 205–211.

† On this and the following points see First Report of the Tidal Harbors' Commission, above referred to, which contains the opinions of our most celebrated engineers on the phenomena presented by tidal and other rivers.

‡ The author has not met with any explanation of the causes that produces changes in river-channels, although the constant alterations taking place in them have been repeatedly alluded to.

Winter-freshets following a sudden fall of rain would raise the water-level of rivers rapidly, and carry it above the banks before the augmented current has time to scour the river-channel and raise it to its former capacity. Accumulations of silt, small at any one place, must each raise the water a little above its proper level, and the point of overflow will be where the sum of these small elevations amounts to more than the height of the banks, above last year's level. But floods leave a deposit of silt, &c. upon the banks they pass over, which increases the capacity of the channel; and until new deposit has again reduced the area of the stream below its proper size, inundation will not occur.

As each flood raises only the part of the bank it flows over, it is easy to see that the point of overflow will be changed from time to time; and every part of the alluvial plains through which a river flows will be visited in turn by floods, provided there are no artificial banks. These banks assist the scouring power of rivers in winter, because they retain more water in the river; but, on the other hand, silt that would have been carried over the banks is kept within the channel, and this may be the reason why the beds of all navigable rivers have become so much elevated during the historical period. The contraction of water-channels in summer, and their enlargement in winter, is thus directly traced to the unequal supply of rain at different periods of the year.

This being admitted, we have an explanation of the manner in which rivers may, by a succession of floods, build upon alluvial deposits along their courses, at the same time raising their beds in proportion to the height of their plains.

If river-channels were perfectly symmetrical in form, the identical sediment that had fallen in summer might be removed again in winter. It is, however, well known that river-channels are deep on one side and shallow on the other. The principal deposit therefore takes place on the shallow or quiet side, and the principal removal occurs from the deep side where the current runs more quickly.

This may explain why the traveller on the Mississippi sees for hundreds of miles a caving bank on one side, and an advancing sand-bar on the other (Lyell). When the action of the river is also unequal on its two banks in different places along its course, a channel consisting of curves instead of straight lines must be produced. When each curve, however, had assumed the complete horse-shoe form, the water, by travelling round the outer circumference of the bend, will have its effective speed reduced to that on the inner or shallow side. The current would thus become more nearly equal in all parts of the channel, and necessarily the deposit likewise; and in winter it would have a nearly equal tendency to excavate the banks on both sides, which condition of equilibrium might last for some time.

Hutton, in 1795, has remarked, that there is evidence of denudation in every country where at any time of the year the streams carry off any particles of the superficial soil.* The Mississippi must derive its vast supplies of mud from thousands of such tributaries; for it could obtain them from no other source, unless we suppose it abstracts them from its own plains. Certainly in many places soil is being removed from one part or other of its plains; but an equal quantity must be added to some other part, for the river could not make a permanent inroad into its plains without enlarging its channel. This it does not do, or it would be able to carry off the winter-freshets without overflowing, and the present artificial bank would be unnecessary.

I have thus briefly referred to observations made by British engineers which may throw some light on the causes of periodical floods, and changes of channel in rivers, and also upon the formation of alluvial plains along their course. These questions need not further be entered into, because the limited growth of alluvial plains and deltas may be best illustrated by tracing the alteration in the mean level of a large part of North America that would be consequent upon a denudation sufficiently extensive to furnish the alluvium said to exist in the valley of the Mississippi. On the borders of the Gulf of Mexico at the present time marine strata are forming within a short distance of the fluviatile, and frequently alternate with them, because spaces of the sea-shore are enclosed by banks of river-mud and converted into lakes ordinarily communicating with the river, but sometimes with the sea after high tides.

The present marine or fluvio-marine deposits must be composed of mud that has passed the mouth of the river, or washed up by the sea, while the freshwater strata must be entirely formed from sand and mud carried over the river banks, or deposited on the bottom of lakes supplied by the stream before it enters the Gulf of Mexico. An idea of the amount of denudation that has taken place in the interior of North America might be either obtained from the extent of the marine deposits formed of mud that had passed the mouth of the river, or from that of the purely fluviatile and contemporaneous deposits formed from mud which had never entered the Gulf of Mexico.

But it is also necessary to estimate what proportion of the total quantity of mud brought down by the river is carried completely out to sea, compared to what is left either upon the marine or fluviatile portion of the delta.

Sir Charles Lyell has remarked, that the alluvium now remaining in the valley of the Mississippi can only represent a fragment

* Our clearest streams run muddy in a flood. The great causes, therefore, for the degradation of mountains never stop as long as there is water to run; although, as the heights of mountains diminish, the progress of their diminution may be more and more retarded. *Op. cit.* vol. ii, p. 205.

of what has passed into the Gulf of Mexico; and this can readily be believed when we reflect upon the depth and breadth of the channel, and upon the short period of the year that the stream would throw any large quantity of mud into the plains even if there were no artificial banks. We must also bear in mind that only the coarse mud could settle near the shore, for the finer particles could not deposit except in very deep water. For these reasons, even if the mud carried beyond the mouth of the river is only ten times the quantity left behind on the fluviatile portion of the delta and plains of the Mississippi, this amount of detritus could not be obtained without the mean level of one-fifth part of North America being reduced 100 feet by denudation affected by the action of rain, the atmosphere, and running water.* But Hutton (vol. ii, p. 401) remarks, in 1795, that wherever any stream carried off particles of soil in its waters at any period of the year, it might be said that denudation was taking place in that country; yet he particularly observed that the waste of land was very unequal, being much more rapid in the elevated than in the more level parts of any district. It is therefore possible that, during the reduction of the mean surface-level of the land drained by the Mississippi to the amount of 100 feet, some portions of the area might be lowered many times that amount, while other portions might suffer little, or be positively raised by the superposition of alluvial deposit. We are, however, informed by Sir Charles Lyell, that the Mississippi in one part of its course cuts through ancient fluviatile beds evidently antecedent to those recent deposits we have been considering. This formation is also stated to contain the remains of species of plants and animals now existing; so that evidence is to be obtained in this district of still greater denudations (by these results) than those of which we have spoken, and which would produce changes on the surface of the earth since the introduction of the present fauna and flora of extent enough almost to realize Hutton's vision of mountains wasted away by the action of rain, the atmosphere, and running-water, and carried along river-courses into the ocean. It is not necessary to take an extreme view of this subject to gain the object we have in view, which is to show that, during the time occupied by the formation of the Mississippi delta, the sea-level might be perceptibly raised† by the agency of physical causes now in operation.

The reasons for supposing that a rise of 3 inches in each period of 10,000 years might occur, have been already discussed, and it only remains to state that, at the present rate of denudation, it

* The data for calculating the annual quantity of detritus carried over the river's banks, in relation with that carried down to the sea, are very imperfect. Further information on this subject is much needed.

† This change of level may amount, under certain circumstances, to a great extent, but at the *lowest* calculation would be 15 feet.

would require five such periods to produce the quantity of detritus said to exist in the valley of the Mississippi; while it would require fifty such periods to produce the requisite quantity of alluvium on the supposition that only one-tenth of the mud in *transitu* through the river was appropriated for the accumulation of its alluvial plains and delta. Under these circumstances it appears a legitimate conclusion, that the level of the sea cannot be considered permanent for all practical purposes when it may be shown that it might be disturbed by the operation of present causes during the period occupied by the construction of a single geological formation. Elevations and subsidences of the land or sea-bottom would also effect important changes in the height of the sea-level, sometimes counteracting and at others adding to the effects produced by the continuous operation of rivers, &c. The effects produced by these important causes would be an additional reason for not considering the sea-level permanent.

It is hardly necessary to add, that the continual waste of the earth's surface by the carrying of materials into the ocean by rivers and breakers particularly attracted the attention of Hutton. He considered* that this was counteracted by elevatory movements of the sea-bottom from time to time, but particularly mentions that it was not necessary to suppose that the dry land was equally extensive at all periods. Since the fluctuation in the sea-level would be directly consequent upon the destruction of land arising from the operation of rain, the atmosphere, and running water on its surface, such changes would be in harmony with the spirit of the Huttonian theory.

Part III.

The average thickness of the deposit formed on the sea-bottom by the solid materials brought on to it from all sources has been estimated in the preceding part of the paper at 3 inches in 10,000 years, producing an elevation of that amount in the sea-level in the same period. Some portion of the oceanic area may be supposed to receive no part of this supply, while other localities nearer the coast-line obtain a great deal more than the average. In the interval between these places, where the rate of deposit is extremely high, and those where it is extremely low, must lie an extensive tract of sea-bottom, where the accumulation of detritus does not much differ from the average rate, which we have supposed to be 3 inches in 10,000 years. Such localities may be more extensive near those parts of the ocean-bottom which receive no supplies of detritus whatever, but they must stretch up to the coast-line in many places. For instance, if it is supposed

* These remarks of Hutton are here introduced because he takes an entirely different view of this subject to that promulgated by Sir Charles Lyell, who considers that there has been always an excess of subsidence. (See Principles, 1850, p. 543.)

that a supply of 10 cubic feet of sand or mud is obtained from each foot of frontage of any coast-line, and distributed between high-water mark and 20 miles distant, it might raise the mean level of that portion of sea-bottom 1 foot in 10,000 years.

Rivers opening on to the shore might also bring down a still greater quantity of material; but although tides and currents are at work removing the sea-bed in one place and forming sedimentary strata in others from the old and new materials, there must everywhere be portions of every sea-bottom where the rate of deposit is intermediate between the highest and lowest, and may often not differ much from that of 3 inches in 10,000 years. These portions of the great oceanic area, wherever they may be situated, are particularly interesting, because on them the accumulation of sedimentary deposit is taking place without any change in the depth of water, and yet without necessitating the supposition of gradual subsidence of the sea-bottom.* Even where deposits are taking place much faster than the mean rate, the variation in the depth of water would be proportionately less than if the sea-level had been permanent.

The limited supply of detritus derived from cliffs, and the wide distribution of that from rivers, renders it difficult to imagine any very extensive tract of sea-bottom where the rate of deposit derived exclusively from new materials should many times exceed the average. Even on areas where extreme cases of denudation and deposition occurred (in periods when the sea-bottom was unaffected by movements, subsidence and elevation), there would be many parts where the condition of depth would remain unaltered, because on them the rise in the sea-level would compensate the addition to the sea-bottom. Also if, in periods that are past, the supplies of detritus from rivers and cliffs were many times greater than at present, they must have caused proportionately greater fluctuation of the sea-level, and therefore under such circumstances there would also be parts of the oceanic area receiving deposit at the same rate that the sea was rising. There would thus have been opportunities for the accumulation of sedimentary rocks without any change taking place in the depth of the water they were formed in, during the intervals when the sea-bottom was undisturbed by subsidences and elevations. For these reasons, in examining the section of a marine formation containing throughout the remains of the same species of Mollusca, it would require independent evidence to determine whether the equal depth of water indicated by the organic remains had been preserved during the formation of the deposit by means of

* The effect of these causes on the general depth of the ocean would be of little importance in a geological point of view, except for *an extended period of time*, such as must have elapsed during the construction of a great serial group of strata.

changes of the level of the sea-bottom, or that of the sea itself, or of both conjointly.

Great caution must also be requisite in judging of the time occupied in the formation of the older rocks from their mineral character, as the following description of passing events will also apply to periods that are long gone by.

Mr. Austen relates in one of his papers, that "with a continued gale from the west large areas of the dredging-grounds on the French coast became at times completely covered up by beds of fine marly sand, such as occurs in the offing, and which becomes so hard that the dredge and sounding-lead make no impression upon it: with the return of the sea to its usual condition, a few tides suffice to remove these accumulations."*

Mr. Deane, the submarine surveyor, also reported to the Institution of Civil Engineers, that the turn of the tide is felt as soon near the sea-bottom at a depth of 120 feet as it is at the surface; and he represents that the loose materials covering the Shambles Rocks are moved backwards and forwards with every tide.

With these facts before us, what criterion can there be (even by estimating the sources of the detritus) for arriving at the minimum or maximum rate at which sands and marls become permanent additions to the sea-bed? For the materials may present all the appearances of hasty accumulation, and yet the interval of time between the deposit of two strata of sand now contiguous may have been occupied by countless temporary deposits, as quickly brought and as quickly removed by the tide, and leaving no trace whatever of their existence. For the same reasons, we cannot be certain that in the valley of the Mississippi we have an unbroken sequence of fluviatile strata, in which the accumulations of one century form the base for those of the next, from the bottom to the top of the series; because there, as in marine formations, the deposits of one period may have been entirely removed in the next. It is therefore possible that many such movements may have occurred, and that the delta of the Mississippi may have occupied a longer period of time for its formation than could be computed from any data remaining. In the preceding part of the paper the conclusion was arrived at, without taking an extreme view of the rapidity with which the materials may have been collected for its deposition, that the work could not have been completed within a period for which the sea-level could be considered permanent.†

There must be, however, many rivers which are only able to afford very small supplies of mud to any alluvial formations, either

* Quart. Journ. Geol. Soc., vol. vi. p. 79.

† It is hoped that in the course of a few years enough data will be forthcoming to determine more nearly the importance of this variation of level in a geological point of view.

from deriving their water from lakes or from countries with a very small rain-fall. During a period when the gradual elevation of the sea-level was not counteracted by the effects of more powerful causes, there would be conditions near the mouths of some rivers of this kind for the surface of their plains to be gradually elevated by the operation of winter floods at a rate somewhat similar to that of the sea-level. In this manner purely fluviatile deposits might be formed in the neighborhood of the ocean, occupying positions similar to that represented in the lower part of the longitudinal section of the Mississippi, without the necessity of supposing any subsidence of the land. In the upper portions of such rivers, the periodical floods, assisted by the accumulation of terrestrial remains in the adjoining plains, would add stratum after stratum during periods when the surface of the country was unaffected by subterranean movements. It is probable that the rate of deposit might be accelerated in periods of subsidence; but the manner in which rivers form plains along their course in all countries under ordinary conditions, when no subsidence or elevation is occurring, was traced by Hutton.

Even if, in ancient periods, the rate of denudation was greater than at present, and the supplies of detritus to rivers more extensive, the fluctuations of the sea-level and the elevation of the beds and plains of rivers would have been proportionately greater. There would, therefore, still have existed some localities where the rate of the formation of alluvial plains near the sea kept pace with the elevation of the waters; so that, as at the present time, conditions would have existed for the accumulation of fluviatile strata containing terrestrial remains without any subsidence of the land. This is a subject, however, that must be further studied, more especially when its value is considered in relation to the great masses of fluviatile strata either of the Mississippi, the Ganges, the Nile, or the Po. For the above reasons it would be difficult to determine, when examining sections of thick fluviatile strata, whether these accumulations of detrital matter had been formed during subsidence of the land, or during the gradual elevation of the level of rivers and seas, arising from the continual operation of ordinary physical causes.

ART. XXVIII.—*On Fuchs's method for the determination of Iron;* by J. R. BRANT.

To determine the amount of iron in any substance, Prof. Fuchs of Munich proposed (Erdmann, xvii, 160) a method which for ease and rapidity of execution is unequalled. It consists simply in boiling the solution of the perchlorid of iron with a strip of bright sheet copper, until the iron is reduced to protochlorid—

the reaction being $Fe_2Cl_3 + 2Cu = 2FeCl + Cu_2Cl$. The quantity of iron is then calculated from the loss of weight of copper, for according to the reaction, as the atomic weight of copper is to that of iron, so is the loss of weight of copper to the quantity of iron sought.

As the idea contained in the method is capable of many analytical applications, it became a matter of interest to determine first the accuracy of the method itself. The iron used in the analyses was fine piano forte wire, and as a preliminary experiment the copper, which was of the purest Lake Superior variety, was boiled for one half hour in concentrated chlorhydric acid, with a loss of 0·69 per cent., and in acid of about one quarter the strength, 0·11 per cent.; showing that the very dilute free acid, in the solution of the perchlorid, can have no sensible effect on the result.

	Iron taken.	Copper dissolved.	Iron found.	
I.	2·0074	2·2010	1·9441 = 96·84	per cent.
II.	2·0591	2·3065	2·0372 = 98·94	"
III.	1·9262	2·1811	1·9265 = 100·01	"
IV.	1·6682	1·8875	1·6671 = 99·93	"
V.	2·2574	2·5045	2·2121 = 97·99	"
VI.	2·0084	2·2855	2·0187 = 100·52	"
VII.	1·9807	2·2015	1·9445 = 98·17	"
VIII.	2·0671	2·3074	2·0380 = 98·59	"
IX.	2·0618	2·3380	2·0651 = 100·16	"
X.	1·0637	1·1907	1·0517 = 98·87	"

The analyses show that although the iron is reduced to protochlorid, the change in color of the solution does not indicate with sufficient sharpness the exact time when such reduction is complete, thus rendering the method inaccurate and unreliable. The method can be made to give accurate results, as soon as some unobjectionable process is given whereby the reduction is rendered manifest independent of mere change in color.

Since the above investigation was completed, a paper on this same subject has been published (Erdmann, lxi, 127) by Dr. Julius Löwe, in which he states that the method, in point of accuracy, leaves nothing to be desired, and gives as proof the following examples:

	I.	II.	III.	IV.	V.	VI.	VII.
Taken,	0·185	0·161	0·126	0·212	0·084	0·282	0·202
Found,	0·179	0·158	0·122	0·212	0·081	0·278	0·201
Difference,	0·006	0·003	0·004	——	0·003	0·004	0·001

Although in these analyses the absolute difference is very small, unfortunately the error in parts per cent. is large; expressed in this manner we have:

	I.	II.	III.	IV.	V.	VI.	VII.
Found,	96·75	98·13	96 82	100 00	96·42	98·58	99·50
Difference,	3·25	1·87	3·18	——	3·58	1·42	0·50

Löwe's own analyses prove then that in point of accuracy the method leaves much to be desired; while by his inconsiderate manner of stating his results he has deceived himself, and probably many others.

New York, June 18th, 1854.

ART. XXIX.—*On Stibiotrizincyle and Stibiobizincyle, two new compounds of Zinc and Antimony, with some remarks on the decomposition of water by the alloys of these metals;* by JOSIAH P. COOKE, Jr., Cambridge.

DURING some experiments on Antimoniuretted Hydrogen, made the last winter, I noticed that the alloys of zinc and antimony, which had been used for preparing that gas, continued to evolve gas from pure water, even after they had been washed completely free from the dilute acid employed in the process. This gas proved to be pure hydrogen, and on boiling the washed alloy with water, I found the evolution so rapid as to recommend the reaction as a process of preparing hydrogen in a state of purity. This fact was announced at the last meeting of the American Association for the Advancement of Science; the new process of preparing hydrogen described, and proofs given of the purity of the gas thus obtained.

On investigating this unexpected reaction, I found that not only the alloys of zinc and antimony, but that also pure zinc would decompose boiling water, before they had been acted on by acids. The following table will show the amount of this decomposition. Column headed I gives the number of centimetres cubes of hydrogen obtained by boiling 200 grammes of different alloys (granulated) with water. The per cent. of antimony contained in the alloys is given at the left hand side of the table, opposite to the number of centimetres cubes. The composition is known only synthetically. The alloys were made by melting together the zinc and antimony of commerce in the required proportions, making no allowance for impurities. The zinc used was shown by analysis to be almost pure; the antimony was a good article of commercial antimony which contained rather over one per cent. of impurities. The antimony contained in the alloys is therefore to be rated at somewhat less than that given in the table according to the per cent. of antimony which the alloys contain. The two metals having been accurately weighed out, were melted together in clean crucibles and the alloys granulated as nearly as possible under the same conditions. Two hundred

grammes of each alloy were boiled with pure water, the gas collected over water, and the number of centimetres cubes evolved in an observed time read off after the gas had been cooled to 20° C. These amounts were afterwards reduced for ten minutes, and thus reduced are given in the table. As it was impossible to obtain the granules of a uniform size in all the alloys, another set of experiments was made in precisely a similar way except that the alloys were cast into small cylinders of a uniform size. As these cylinders had absolutely the same diameter, and almost the same specific gravity throughout, the same amount of surface was obtained by weighing out 200 grammes of each alloy, and taking care to have the same number of little cylinders in each lot. Column 3 gives the results of these experiments, where of course the same correction for impurities in the antimony must be made in the composition of the alloys. It will be seen that the two sets of numbers compare as closely as could be expected, it being remembered that the amount of surface in the first set of experiments was variable, while that in the second was constant and smaller than the first. These results however must be regarded only as approximations to the truth. The limits of variation in different experiments on the same alloy would quite cover the differences between the first ten numbers of column one, excepting the first, so difficult is it to granulate the alloys to a uniform size, and submit them during the experiments to precisely similar conditions. The numbers of column 3 from which the variations due to difference of surface have been eliminated, are probably relatively to each other very nearly correct.

Table of the Amounts of Hydrogen Gas evolved by 200 grammes of different alloys of Sb and Zn, in ten minutes, at 100° C. measured at about 20° C.

Per Cent. of Sb.	1.	2.	3.
0	2	63	
5	6	34	
10	4	28	3
15	4		
20	6	18	5
25	4	19	
30	4	31	5
35	5	49	
40	6	72	7
45	5	45	
50	8	44	9
55	17	46	
58	130	244	84
60	50	139	47
65	14	35	
70	10	45	7
75	6	36	
80	5	23	6
85	4	20	

A mere glance at the table will discover two facts:

1st. That up to 50 p. c. no great increase in the amount of hydrogen evolved is obtained by increasing the amount of antimony in the alloy.

2nd. That at the alloy containing 58 p. c. of commercial antimony, or about 57 p. c. of pure antimony, there is an immense maximum which is confined between at most two per cent. on either side.

Before passing to the result to which the last of these facts directly points, I will briefly state the few additional facts which I have observed in regard to the decomposition of water by the antimony alloys.

It is a well known fact that the rapidity of the evolution of hydrogen from dilute sulphuric acid and zinc can be very greatly increased by adding to the materials a few drops of a solution of chlorid of platinum. The platinum being immediately deposited on the zinc, forms with it a galvanic pair, and thus increases the affinity of the zinc for oxygen. The same increased action can be produced by the same means in the decomposition of pure water by the antimony alloys. Column 2 of the table gives the results which were obtained by boiling with pure water in a small flask 200 grammes of the granulated alloys, previously treated with the same amount in each case of a solution of chlorid of platinum. After the platinum had been deposited on the granules and the surfaces had been thus blackened, the alloys were thoroughly washed with water and the experiments conducted as in the other two cases. These experiments were made with the same alloys as those from which the numbers of column one were obtained. As however in the experiments with chlorid of platinum new and obvious causes of irregularity were introduced that did not exist in the other two sets of experiments, no great uniformity can be expected on comparing the results. The two main facts however noticed in columns 1 and 3 of the table are quite as prominent in column 2, and also the additional fact that the presence of platinum very greatly increases the rapidity of the evolution of hydrogen from the alloys.

One set of results given in the table requires particular notice; those obtained from pure zinc to be found on the first line opposite 0 p. c. of antimony. It is stated with great confidence by all chemical authors who have written on the subject that Zn does not decompose water at the boiling temperature. On this account the experiments with pure zinc were made with peculiar care and repeated several times, great pains being taken to ensure that both the zinc and water employed were perfectly pure. There is no doubt in regard to the fact of the decomposition which becomes, as is shown in the table, quite rapid when the affinity of the zinc is strengthened by the galvanic action of the platinum.

When the alloys of zinc and antimony are treated with strong acids, hydrochloric or sulphuric, they are as a general rule, and under favorable circumstances, completely decomposed, the zinc uniting with the acid and the greater part of the antimony separating as a black powder, only a very small amount ever, even under the most favorable circumstances, escapes as antimoniuretted hydrogen. When the alloys are in granules it is almost invariably the case with those which contain more than 50 per cent. of antimony that after a short time the acid ceases to act, owing to the formation of a coating of antimony on the surface. The action is of course renewed on reducing the alloy to powder, but here as in other alloys, the less oxydizable metal appears to be able to protect entirely a certain amount of the other from the action of acids.

These facts in connection with those previously stated in regard to the increased action of the alloys on water in presence of platinum sufficiently explain the remarkably rapid decomposition of water obtained by means of alloys which have been previously acted upon by hydrochloric or sulphuric acids, even after the excess of acid and the salts formed have been completely removed by repeated washings. This decomposition is so rapid that I have obtained from 200 grammes of an alloy containing 58 per cent. of antimony prepared as just described and boiled with water, nearly a litre of gas in ten minutes. It is plain that the antimony acts here exactly as the platinum in the previous experiments by forming a galvanic circuit with the alloy. A set of experiments was made with alloys which had been acted upon by acids similar to those the results of which are given in the table. The irregularities however which resulted from the unequal action of the acids on the different alloys, from the differences of surface and from other causes rendered the final results so discordant that they were of no value for comparison. They were always much greater than those obtained by using platinum, with the exception of pure zinc, whose decomposing power was not increased by the action of acids.

This new mode of decomposing water is of value as a process for preparing pure hydrogen, and also for illustrating the composition of water to a class. When the antimony and zinc used are free from arsenic, and the water not in actual ebullition, the hydrogen obtained is chemically pure. If commercial antimony and zinc are used, the gas will be found contaminated with a small amount of arseniuretted hydrogen, so small however as to be with difficulty detected, and entirely inappreciable in the most refined eudiometric experiments.

Gas evolved from an alloy containing 50 per cent. of commercial antimony was burnt in Regnault's eudiometer with the following results:

Tension of hydrogen used, - -	0·379	metres.
Tension of hydrogen + oxygen, -	1·219	"
Tension after combustion, - -	·653	"
Tension of gas consumed, - -	0·566	"

$0{\cdot}566 \times \frac{2}{3} = 0{\cdot}378$, amount of hydrogen consumed.

My mode of preparing the alloy for making hydrogen is simply thus: I melt together equal parts of zinc and antimony (this alloy being nearly or quite as active after having been treated with acid as the 58 p. c. of antimony) and granulate as finely as possible. I place the granules in a deep porcelain basin, and pour over them enough hydrochloric acid of ordinary strength to cover them. An energetic action ensues, which I allow to continue until it becomes weak and the acid nearly exhausted. The excess of acid and also the chlorid of zinc formed, I now wash away by allowing a stream of water to pour into the basin until it runs off clear and tasteless. The alloy thus prepared is ready for use. It will evolve hydrogen from boiling water with almost as much rapidity as zinc and dilute sulphuric acid will in the ordinary process, and even after the temperature of the water has fallen to that of the air, the evolution continues though only very slowly. A flask containing about a pound of the prepared alloy covered with water, continued to evolve hydrogen during the last winter for over two months, where the temperature was seldom above 4° C.

The rapidity of the evolution of hydrogen from the alloy and boiling water diminishes quite rapidly and finally after several hours ceases altogether from the formation of a coating of oxyd on the surface. The activity of the alloy can be restored by dissolving off this coating with dilute acids; where however the alloys contain a large per cent. of antimony (above 50) the activity can not be renewed indefinitely in this way since the particles of antimony set free by the acid adhere to the surface of the alloy and soon form a coating impregnable to the strongest acids. For these reasons this process will not be found economical for preparing hydrogen in large quantities, although I think it will be useful where the gas is desired chemically pure.

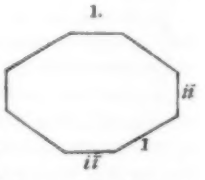
The large maximum which was observed in the table opposite 58 p. c. of antimony indicated the existence at that point of a definite compound. The true composition of this alloy, considering the impurities of the antimony, was nearly Zinc 43 p. c., Antimony 57 = 100, which corresponds almost precisely to the symbol $Sb\,Zn_3$. The compound which this symbol represents I will term, following the analogy of the nomenclature adopted by the German chemists for similar compounds,

Stibiotrizincyle.

It can be obtained by melting together 58 p. c. of commercial antimony and 42 p. c. of zinc, and allowing the liquid mass when thoroughly melted together, to cool until a crust forms on the surface. On piercing through this crust and turning out the still liquid alloy, the crucible if broken open when cooled will be found filled with the most beautiful prismatic crystals. In order to obtain crystals of any size, it is necessary to use eight or ten pounds of the alloy, and cool the crucible very slowly in sand. These crystals present the following properties. The isolated crystals are small, a few tenths of a line only in diameter and not generally over an inch in length. They tend however to form compound crystals with parallel major axes which are often several inches in length and a quarter of an inch in diameter. Naturally they present a silver white color and a beautiful metallic lustre. The surfaces are often however iridescent, owing to a slight oxydation, and the true color is then only seen on the fracture. Sp. Gr. of crystals = 6·48, Homer.

Their form is that of a rhombic prism, with sometimes only one, but generally with both sets of edges truncated. A section through the lateral axes is given below with the angles between the planes of the prism. The crystals invariably, so far as I have observed, run out to fine points, and although I have examined many hundreds of these crystals, I have never seen one with a termination.

I on $i\bar{i} = 148° 30'$
I on $i\bar{i} = 121° 30'$
I on I over $i\bar{i} = 117°$



I have observed variations from the angles given above on crystals of the same crystallization amounting to ten minutes. The angles given measured the same to a minute on crystals from three different crystallizations, and are therefore regarded as the most probable.

The composition of the crystals obtained as above was found on analysis to correspond very closely to $Sb\,Zn_3$. Of three analyses made by myself of crystals from different crystallizations, obtained by melting together 58 per cent. of commercial antimony and 42 per cent. of zinc, the greatest difference between either the zinc or the antimony found and that required theoretically was five-tenths of a per cent. If we assume that the atomic weight of zinc = 32·53, and that of antimony = 129·03, which accords with the best recent determinations, then the composition of $Sb\,Zn_3$ is as shown in column 1 below. Columns 2, 3, and 4 give the results of the three analyses just mentioned:

	1.	2.	3.	4.
Antimony,	56·94	57·24	56·50	56·93
Zinc,	43·06	42·83	43·06	43·15
	100·00	100·07	99·56	100·08

There can be no doubt therefore that an alloy which contains 57 per cent. of antimony and 43 per cent. of zinc will give crystals which have a composition corresponding to $SbZn_3$.

It was found however that the same prismatic crystals could be obtained from melted alloys which contained proportionally a much larger amount of zinc, but not from those which contained less. As the amount of zinc in the alloy was increased, the crystals became less and less abundant, until they seemed to fade out when the amount had been increased to about 84 per cent. A series of analyses were made in order to ascertain how far the composition of the melted alloy influenced the composition of the crystals which were formed in it. The results of these analyses are given in the following table. In the left hand column are given the per cents. of zinc in the alloys from which the crystals crystallized. In the right the per cents. of zinc found in these crystals on analysis. With a few exceptions in these analyses the zinc only was determined. The zinc per cents. marked with my name however, are from complete analyses.

Per cent. of zinc in the alloy.	Per cent. of zinc found in crystals.
42 per cent.	43·09 *Cooke.*
43 "	44·14 *Cooke.*
44 "	44·26 *Eliot.*
46 "	46·77 *Eliot.*
48 "	48·66 *Eliot.*
50 "	46·89 *Cooke.*
52 "	47·28 *Homer.*

Here as in the former table the composition of the alloys is only known by synthesis, and as commercial antimony was used for making them, the per cent. of zinc in the left hand column is nearly one too low in each case. Considering this it will be seen that the crystals have the same composition as the alloy up to 49 per cent. of zinc, but that after having taken up 6 per cent. excess of zinc, they seem incapable of taking up any more, so that when the per cent. of zinc is further increased in the alloy, it falls off in the crystals, and the alloy of 53 per cent. of zinc gives crystals which contain one per cent. less zinc than those obtained from alloy of 49 per cent. It is unnecessary to say that well defined crystals were always selected for analysis.

Crystals were measured from most of the different alloys of the last table, and were found to have the same form considering the variation already noticed, as the one figured and described. I

was not however able to obtain crystals from alloys either of 49 p. c. or 53 p. c. of zinc, whose angles could be accurately measured.

This result is certainly very remarkable and important in its theoretical bearings, and will be the more so should it be found that the same variations in composition appear in the compounds. Until I have investigated these I shall refrain from advancing my views on the subject. The facts just stated are substantiated by a very large number of analyses and measurements besides those which appear in this paper.

Stibiobizincyle.

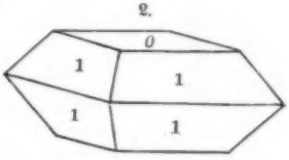
This compound may be easily prepared like the last by crystallizing an alloy containing about 33 per cent. of zinc, and 67 per cent. of antimony. In its natural state like Stibiotrizincyle, it has a silver white color, and a very bright metallic lustre, often however its surfaces display prismatic colors owing to oxydation. It forms in right rhombic octahedrons with basal planes of the Trimetric System. Here as in the other crystals, I have observed variations in the angles amounting to 20 minutes between the extremes. The crystals are frequently very perfect and their faces so plane and bright, that the angles can be measured to a minute. The angles given were all obtained by measurement, except the one over X, which measured six minutes more than that required by the other two. These angles are nearly the mean of those observed.

O on 1 = 122° 15′ measured on each side.

1 on 1 over Z = 115° 30′, measured.

1 on 1 over Y = 118° 24′, measured.

1 on 1 over X = 95° 24′, measured 95° 30′.



Axes $a = 1$, $b = 1{\cdot}042$, $c = 0{\cdot}793$.

These crystals were analyzed by Mr. Eliot with the following results:

Analysis.		Theoretical $SbZn_2$.	
Zinc	= 32·52	Zinc	33·55
Antimony	= 66·86	Antimony	66·45
	99·38		100·00

I have now given an abstract of the results on these two new compounds which I have obtained up to this time. I am now engaged in investigating their chemical relations and compounds. The results of this investigation I hope to be able to publish during the Autumn, in the form of a memoir, to which I must re-

fer for the details and proofs in relation to many points which have been stated in this paper. In concluding, I would express my warmest thanks to Mr. Charles W. Eliot, Tutor in Harvard College, and Mr. Charles S. Homer, assistant in my laboratory, for their assistance and zeal in prosecuting the investigation. Their names have already appeared in the course of the paper.

ART. XXX.—*On the Nature of Forces;* by Lieut. E. B. HUNT, Corps of Engineers, U. S. A.*

IT is a peculiarly significant fact that all the great agencies of Nature which act from local centres or origins through sensible distances, follow the Newtonian law of variation in intensity with an inverse duplicate function of the distance. In this respect, light, radiant heat, sound, gravitation, electric and magnetic repulsions and attractions all agree. As the so-called forces of cohesion, elasticity, chemical affinity, electrolysis, crystallization, capillarity, friction, &c. exhibit sensible actions only at insensible distances, their laws of variation in intensity with varying distances of action are incapable of direct determination. We do not even know if these agencies are original and primary, or resultant and secondary forces. As inertia acts only in the ultimate units of matter, it cannot be supposed to be at all connected with distance. The mutual action of galvanic currents, in which both distance and inclination affect the attraction or repulsion, is clearly a complex result of the transmissive motion of the currents, and forms no static exception to the Newtonian law. It seems therefore a general fact that all primary natural agencies, which act from central origins through sensible distances, are embraced under one mathematical formula, which is that expressed in the all-embracing Newtonian law.

What then is the rational translation or philosophical significance of this law? To this question one answer may be given, which fully illustrates that crowning simplicity so uniformly characterizing natural facts of the highest generality. Newton's law rigorously expresses the necessary facts of simple outward emanation or procession from a central origin. Whatever case of agency emanating from a centre we may suppose, whether it be light, radiant heat, sound, force, or any other, this fact of emanation makes the Newtonian law an inevitable result. This law simply expresses the fact that the agency in question undergoes neither increase nor diminution by outward transmission. If any other than the inverse duplicate ratio be supposed, it must involve

* Read before the American Association for the Advancement of Science, at Washington, May, 1854.

either an increase or diminution of the aggregate agency, as consequent on mere transmission through space. But it is clear that mere transmission is totally incapable in itself of affecting in the slightest degree the quantity of action originally put forth from the centre. Mere change of place cannot, by its very nature, be a producing or destroying cause. The inertia, the structure, the imperfect elasticity of the transmitting medium, may produce a decay of transmitted action, as in the case of light in an imperfect medium or of heat in air; but mere transmission as such, is as wholly powerless to destroy as it is to create action.

The more clearly to perceive that unresisted central emanation necessarily gives the Newtonian law, let us conceive a centre from which action of any kind issues or emanates by rectilinear radiation. Each ray throughout its entire length is the representative of the same quantity of action. Now if we suppose elementary concentric spheres around this origin as a centre, each sphere is pierced by all the rays and hence all the spheres become *loci* of the same amount of total agency in a given time. If the emanation or radiation be supposed uniform in all directions, then the total intensity of action on each unit of surface for any particular sphere, is inversely as its total surface, which is as the square of the distance of transmission. Hence the action on a given surface, or a given constant mass, is inversely as the square of the distance of transmission. Or, instead of rays, we may suppose the emanation to proceed by spherical undulations, where a like train of reasoning will lead to a like result. The two mechanisms of radiation and of spherical undulation concur in giving the Newtonian law, as the necessary expression of unobstructed emanation, the law being indeed but a simple assertion that the emanating agency is neither increased nor diminished by outward propagation or that translation through space, neither makes nor destroys light, heat, force, &c. The same facts in a negative order would characterize a central absorption of agency.

Since free emanation thus leads to the Newtonian law as a necessity, the reverse question arises: whether the existence of the Newtonian law does not of necessity involve emanation? It surely furnishes a powerful evidence of emanation, but is not a positive proof of it; for we can suppose the exact geometrical system of dynamic agency which emanation produces, to be by the original creation and constitution of matter, embodied in an identical static form. For instance, we may suppose an atom so constituted as to fill with its actual and organic self all the space to which its force action would extend, and thus to have everywhere a potentiality identical with that resulting from true emanation. This hypothesis literally makes each atom fill all space, and all atoms actually to coexist in each point of the universe. Thus too, if one atom be moved by its centre, it must every-

where move through every other atom. We are mathematically compelled either to admit this strange universal coexistence of all atoms in each point of space, each atom being everywhere truly distinct, or else to ascribe to an emanation from central points or nuclei, all the forces which follow the Newtonian law. But the exceeding improbability of this coexistence theory may best be estimated by inquiring into the chance of an original static creation being based on the inverse duplicate ratio. *A priori*, this particular ratio has no preeminence of probability over any *sub* or *super* ratio of variation. We may indeed ascribe the existence in Nature of this exact ratio to an intelligent Divine choice or selection, but as a question of chances, it is as infinity to one that some other ratio would have existed.

Between these two conceptions, each of which is a geometrical possibility, this consideration of chances almost compels us to choose the idea of emanation. When too we consider the exceeding complexity of mechanism and the great metaphysical difficulties involved in the idea of coexistence, and when we observe that the inertia of each atom appears thus to be diffused through the entire universe, the coexistence theory seems a hypothesis of the least promising character. For these reasons the existence of Newton's law in any type of force or other agency, seems legitimately and almost by constraint to be referable directly to the emanative outward transmission of the force or agency from its originating central points or nuclei. As all known primary forces do in fact follow this law when acting through sensible distances, the inference follows that all these forces are actually emanative.

But are all primary forces necessarily emanative? Certainly not: yet we ought not, except as a last resort, to hypothecate forces *not* emanative, as such a hypothesis is unwarranted by our actual knowledge. All such hypotheses involve generic force types, unlike that one which includes all forces whose laws are really known to us. Before assuming primary forces, varying inversely with the 1st, 3d or 14th powers of the distance, we are bound to exhaust all the resources for phenomenal interpretation, offered by forces following the Newtonian law. To assume that the same primary force is attractive at one distance and repulsive at another is like saying that yes becomes no by a change of latitude. The expedient of leading one primary force through various alternations of attraction and repulsion, as is apparently done in the theory of spheres of force, must to a reasoning mind appear too conveniently Protean and time-serving to be accepted as any thing better than the fig-leaf of our ignorance. We really know of but one type of force, and that one has a law which means emanation; yet speculation has run riot among all possible ratios of force decrease, and the *force entity* has been treated as a

shuttlecock between attraction and repulsion, just as present convenience dictated. We must have a more grand and simple idea of force, ere the labyrinth of molecular mechanics will yield its clue. In molecular studies, there is a strong and widespread tendency to complex hypotheses which but ill accord with the fundamental simplicity of Nature, and which by hiding our ignorance, effectively retard our progress towards knowledge. To exorcise this tendency would greatly promote the consistent extension of strict mechanical investigation over the rich fields of molecular constitution.

With a view to developing the principles now presented and as a preliminary to some discussion of the theoretical views advanced by Boscovich and Faraday, I will here proceed to develop a few of the properties of central forces varying with an inverse function of the distance, and which may be either emanative or static by coexistence.

Assume a centre of force (or other agency) at an origin of rectangular coördinates, and conceive the force to be radiated uniformly in all directions, each ray being in its entire length the representative of a constant intensity of action, or of an agency varying in intensity with any inverse function of the distance. This mechanism must it is evident, give results identical with those which would result from a corresponding spherical wave mechanism. Suppose now a circular disc to advance or recede relative to the origin, by being moved along the axis of **X** by its centre and being maintained perpendicular to it; the reception of rays by this disc will be a measure of effect so long as the obliquity of these rays can be disregarded. Calling the force or aggregate action y, when the disc is at the distance x from the origin, and y' when it is at the distance unity; we have $y' : y :: x^2 : 1$, or $y = \frac{y'}{x^2}$. If now we conceive each ray as having an intensity varying with a simple inverse function of x, we shall obtain $y = \frac{y'}{x^n}$, in which n exceeds by two the exponent of variation along each ray.

If we differentiate the equation $y = \frac{y'}{x^2}$, regarding y as a function of x, we obtain $dy = \frac{-2y'dx}{x^3}$, and this is the force decrement corresponding to the Newtonian law. This can best be appreciated by deriving it directly. Let the disc advance towards the origin through a distance $= dx$; the increase of ray reception by the disc, or the differential of the force is found by determining the elementary ring projected around the former position of the disc. Calling the disc radius r, and the width of the added ring dr, we shall have by proportionality $x : -dx :: r : dr$,

and $dr=\frac{-rdx}{x}$, also $y:dy::\pi r^2:\pi(r+dr)^2-\pi r^2$. Hence by reduction and by neglecting dr^2 as an infinitely small quantity of the second order, we obtain $dy=\frac{-2ydr}{r}=\frac{-2ydx}{x}=\frac{-2y'dx}{x^3}$, which is the expresssion above found as the differential of a Newtonian force. The signs of dx, dy and dr depend on the direction of the motion of the receptive surface.

It will be seen by inspecting the above, that not only is Newton's law derived from this consideration of ray-reception, but that the differential equation of that law expresses simply the relation between the differential of distance and that of ray reception. This law also involves the two assumptions which for all appreciable distances are entirely admissible, though not at all so for extremely small distances: first, that the effect of ray obliquity for the same receiving disc may be neglected, and second, that the diffusion over the disc may be regarded as uniform. By substituting spherical atoms for the disc, we at once obtain the case of nature, when the question is of actions between sensible masses.

If we construct the curve of the equation $y=\frac{y'}{x^2}$, we find that both axes of coördinates are asymptotes to its branches or that the force ordinate is infinite at the origin and zero at an infinite distance. But the most remarkable fact in this class of curves is that each particular radius of receiving disc corresponds to a particular curve. When the distance abscissa equals the disc radius, the differentials of force and distance are always equal in construction. The law of the increment is only satisfied by that curve which cuts the line through the origin making an angle of 45° with the two axes, in the point whose ordinate equals the radius of disc. Thus the radius of the receptive disc or atom determines the particular characteristic curve of relation between force and distance: a curve which is the same for a homogeneous sphere of atoms as for a single component atom.

Passing to the more general function $y=\frac{y'}{x^n}$, and differentiating, we obtain $dy=\frac{-ny'dx}{x^{n+1}}$, in which if we substitute y for $\frac{y'}{x^n}$, we obtain $dy=-ndx\frac{y}{x}$. If we suppose now that all the curves corresponding to any particular value of n are duly constructed, and if a right line through the origin make the angle x with the axis of x then $\frac{y}{x}=$ tang. $\alpha=$ a constant; or in $dy=-ndx\frac{y}{x}$, we

find a constant of force increment, for all the points in which a straight line from the origin cuts the various curves of the system. The significance of this result is obvious when we consider that radiation gives shape to the formula. By farther discussion, it would be seen that the increment for a given ordinate varies inversely with the abscissas and directly with the ordinate for a given abscissa.

If the series of parallel curves corresponding to $y=\frac{y'}{x^2}$ be constructed for all values of y' from *plus* infinity to *minus* infinity any possible attraction or repulsion curve for which the force varies as $\frac{1}{x^2}$ will coincide with some one of this series. No two curves of this series when referred to the same origin and axes can be made to intersect. This property is general for all central force curves, in which y varies as $\frac{1}{x^n}$ taken in sets for each value of n from zero to *plus* infinity. This signifies that if any number of central forces acting from the same centre according to the same law are in equilibrio at one point, they must be so at all distances. Also if any number of attractions and repulsions act from one point as $\frac{1}{x^n}$, their resultant also acts as $\frac{1}{x^n}$. Thus

$$\frac{y_{a'}+y_{a''}+y_{a'''}+\text{ \&c. }-y_{r'}-y_{r''}-y_{r'''}-\text{ \&c. }}{x^n}=\frac{y'_{a'}+y'_{a''}+y'_{a'''}+\text{ \&c. }-y'_{r'}-y'_{r''}-y'_{r'''}-\text{ \&c. }}{x^n}=ya \text{ or } yr=\frac{y'a \text{ or } y'r}{x^n}$$

which is of the original form. Hence all forces emanating from a centre and varying as $\frac{1}{x^2}$ are equivalent to a single resultant varying as $\frac{1}{x^2}$ which is wholly attractive or wholly repulsive: and the same for each value of n. Hence a medium composed of one species of atoms endowed with forces varying according to Newton's law must be unalterably compacted or unalterably elastic. Therefore a single type of atoms with Newtonian forces cannot represent the facts of nature, and we must either suppose other forces than those varying as $\frac{1}{x^2}$ or we must suppose more than one kind of matter.

If we suppose an atom to exercise two central forces, one attractive and one repulsive for which n has different values, their curves will intersect or the forces will balance at one and only one distance. Thus if the ordinary attraction, $y_a=\frac{y'_a}{x^2}$ and any

repulsion $y_r = \frac{y'_r}{x^n}$, act from the same centre their resultant, $y_a - y_r = \frac{y'_a x^{n-3} - y'_r}{x^n}$, can only be zero when $x =$ infinity, or when $x = \sqrt[n-3]{\frac{y'_r}{y'_a}}$. In general $x = \sqrt[n'-n'']{\frac{y'_r}{y'_a}}$, is the abscissa or radius of the point or sphere of equilibrium of these two forces. Hence such a primary attraction and a primary repulsion acting between simple atoms, can together give but one type or form of equilibrium, and thus must fail to give the solid, liquid and gaseous conditions of aggregations. Besides two forces involving different laws of action or values of n, can in no wise give the simple Newtonian form as it appears in gravitation. From this we can say with confidence that heat or the interatomic repulsive force is no mere radial repulsion varying as $\frac{1}{x^n}$.

To suppose three or more distinct atomic forces varying as $\frac{1}{x^{n'}}$, $\frac{1}{x^{n''}}$ and $\frac{1}{x^{n'''}}$, or to suppose a single force following no simple functional law, but being now repulsive, now attractive, now infinite and again Newtonian, is to give ourselves up to bewilderment and to achieve a chaos of explanation. Simple emanation and its resulting law bear *a priori* credentials of being the great facts of dynamic nature, and until they are shown to fail entirely in the exposition of molecular mechanics, a resort to other conceptions must be considered illogical and unhopeful. The hypothesis of two distinct species or kinds of matter, with original forces peculiar to each, but all following the Newtonian law, is far more promising than any other, and Mossotti has to some extent shown its ample power of explanation. One important element is overlooked in his analysis, nor has he developed the mechanical effects of heat on aggregation, though in this respect his views are a vast advance on the total neglect of heat by Boscovich's theory—as also on the expositions by Poisson.

From the point which this discussion has now reached, I wish to examine somewhat the theory of Boscovich and the speculation of Faraday, freely stating some objections not hitherto urged.

Boscovich by denying size to atoms, prevents them from presenting any material surface or volume on which to receive force action. If forces emanate from the atoms of a mass A, and penetrate the volume of a mass B, the atoms of the mass B would not according to the Boscovich theory, present any surface or volume whatever on which to receive the force action. How then is the Newtonian law, or indeed any action to be derived? As this law but expresses the condition of ray-reception, what

does it mean when this reception is precluded by the total lack of magnitude in atoms? Nothing remains but to conceive the force rays or their equivalents from the atoms in *B* as every where receiving the action of the intersecting rays from the atoms in *A*, and referring these actions back to their own centres. Now unless these rays are conceived as possessing magnitude, and as actually filling all space, the result of ray-intersections, depending as it must on the number of intersections, would not be the exact Newtonian law, but one essentially departing from this, by a difference which increases as the rays from each atom are supposed less completely to fill all space. Thus to obtain the Newtonian law, we appear to be driven again to that strange hypothesis of each atom filling all space and all atoms coexisting in each point, and to require still other special conditions; all simply as a consequence of denying size to atomic nuclei. As the power of receptivity must exist either in atoms of finite size or in atoms of infinite size, and as we must either locate inertia in a nucleal atom or in an infinite one; we seem quite justified in preferring emanation from and reception by definite nucleal atoms to the bold hypothesis of a static entity, activity and inertia belonging to infinite coexistent atoms. If we attempt to conceive a material mass, as a wall for instance, according to this coexistence theory, we shall find it signally inadequate to the realization of facts to the mind, which though not a logical objection, is a serious practical drawback. But by conceiving atoms as solid, impenetrable, definite volumes, from which force incessantly emanates, and by which force is incessantly received, the mental difficulties wane away, and matter becomes to the mind a localized reality. However small the atomic volumes be assumed, so long as they have a real and finite size, a receptive capacity and the Newtonian law result at once.

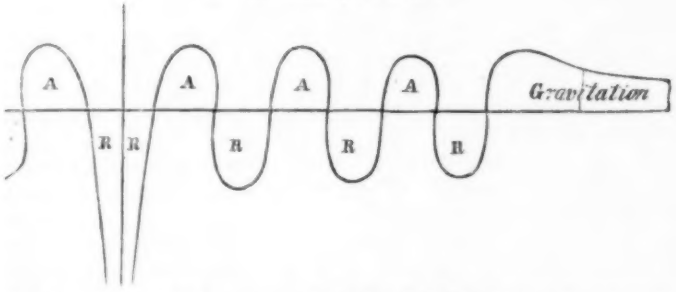
If I rightly apprehend Faraday's views (Phil. Mag., Nos. 157 and 188), they are such as would give a law quite different from Newton's. The interactions of rays conditioned as he supposes, could only give the actual result by so extending the amplitude and number of rays as that all points of space should be points of interaction between the rays of each atom and of all other atoms which is the coexistence theory again. To deduce the actual law from the views so modestly set forth by this excellent investigator, would, I think, be a mathematical impossibility; to say nothing of their inadequacy as they now stand, to serve the cause of molecular mechanics. The objection to the views of Boscovich and Faraday on the ground of their not providing for inertia has been well urged by Airy (Phil. Mag., No. 190).

There is another signal fault of the Boscovich theory, which at this time is peculiarly objectionable. While its mechanism is empirically devised with special reference to the solid, liquid and

gaseous states of aggregation, it really takes no account of heat, but at once refers these states to primary forces assumed for the purpose. Yet it is certainly the degree of heat, and that alone, which in fact mainly determines these forms of material existence. If any relation is supposable between heat and the force spheres of Boscovich, it remains to be discovered what it may be. But as the theory now stands, heat is ignored, and force spheres usurp the work actually performed by heat. This theoretical false causation is a positive stumbling-block in the way to clearer views of heat and molecular aggregation. The more we reflect on the wide range of actions due to heat, the more incompetent to their representation will we find the conception of primary spheres of force. In nature, aggregation is actually almost absolutely ruled by temperature, yet nothing at all like temperature seems legitimately derivable from the Boscovich theory. This theory indeed requires a mass of any given substance when not compressed, always to fill exactly the space given by its atoms being in positions of mutual indifference, and does not provide for expansions and contractions through heat variations.

I will now state a striking proof that the views of Boscovich have received their present somewhat wide acceptance among men of science, without being subjected to that criticism, analysis and comparison with nature, which alone can entitle them to any authority or weight. There is a glaring fundamental oversight involved in his theory as it now stands, and as it is figured in that exponential curve so frequently found in works on this subject, and even in mechanical text-books. This oversight con-

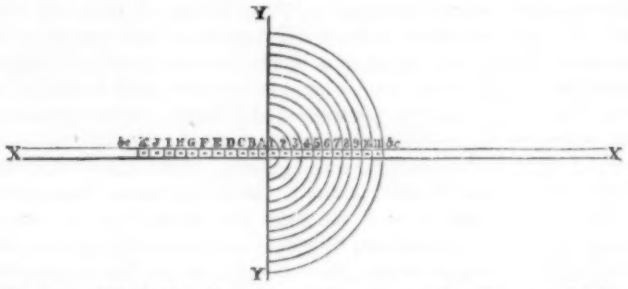
1.—*Curve of Boscovich.*



sists in his not having correctly extended his theory to masses of matter, which, unfortunately for his theory, is the only case occurring in nature. Taking his exponential curve of force between two atoms as it stands, and discussing a mass composed of such atoms, it appears that instead of the various types of aggregation and force manifestation hitherto supposed to result, there will be but a single cohesive type, which will be invariable for each mass.

To illustrate this, let us assume along the axis of X, (fig. 2), a line or thread of atoms, at mutual distances corresponding to a

2.



particular solid or liquid, and acting on each other according to the Boscovich force curve: call the atoms to the left of the origin A, B, C, D, &c., and those to the right, 1, 2, 3, 4, &c. In Vol. I. of Robison's Mechanical Philosophy, there is an exposition of Boscovich's theory, to which most of his disciples are indebted for their acquaintance with his views, and in this, the widest limit of cohesion is fixed at about one-thousandth of an inch, within which are several alternations of attraction and repulsion branches. Now by comparing this distance with the almost infinitely minuter threads, membranes, eye-points, &c. revealed by the microscope in infusorial and other organic forms, or with any of the countless facts showing the extreme divisibility of matter and the differential character of interatomic distances, it will become evident that many thousands of atoms lie within this outer limit of cohesion, or in the one-thousandth of an inch measured in a mass. Boscovich in his Theoria leaves the case essentially in the same condition. Hence the atoms A, B, C, D, &c., on the left of the origin act on 1, 2, 3, 4, &c. on the right of the origin, the attractions and repulsions alternating through many thousands of atoms on each hand. The atom A repels or attracts 1, 2, 3, 4, &c. according to distance, all atoms exterior to the last limit of cohesion being attracted. Thus in the force curve for A, many atoms will be found to correspond to each branch. We may therefore, with almost perfect accuracy regard the areas between the several branches and the axis of X, as proportional respectively to the corresponding aggregate actions of A on 1, 2, 3, 4, &c. The action of B on 1, 2, 3, 4, &c. is analogous, and so of C, D, &c., a portion of the curve towards the origin being cut off for each receding atom. Now on examining the areas between the different branches of the curve as drawn in Boscovich's Theoria, Robison's Mechanical Philosophy, Daubeney's Atomic Theory, Bartlett's Mechanics, &c., it will be seen that the outermost area, corresponding to gravitation, is very much greater than that for

the adjacent repulsion branch, and that the successive attraction and repulsion branches embrace about equal areas. Hence the attraction between A and the gravitating atoms in the line is decidedly greater in its aggregate than the sum of the adjacent repulsions; as is amply realized when we consider that the attraction area extends to infinity between the curve and the asymptote axis. Hence the action of A would be to draw the gravitating part of the column towards itself with a much greater force than it repels the adjacent portion, so that a large surplus of attractive pressure is passed along the column to the next attractive branch, which is thus made greatly to surpass the next repulsion and so on through the whole curve, until the interior repulsion is reached, where the aggregate attractive surplus is balanced by the final indefinite repulsion. The action of B, C, D, &c. is entirely similar, except in the successive pruning of the inner extremity of the curve. Hence the aggregate action of A, B, C, D, &c. on 1, 2, 3, 4, &c. is simply a prevailing attraction which is only effectively resisted by the final indefinite repulsion, and thus the whole mechanism of this curve serves only to make some perturbations in attraction with no palpable result whatever.

Passing now from a line of atoms to a medium or mass of matter, the same result is found only vastly exaggerated. Referring the medium to three rectangular axes of X, Y, Z, and conceiving the line of atoms already discussed as coinciding with the axis of X, we wish to determine the aggregate forces which counteract each other in a superficial unit of the plane Y Z. The total action of the column A, B, C, D, &c. on the matter filling the space beyond the plane Y Z, affords the true criterion of forces acting in a medium, since all the columns parallel to A, B, C, D, &c. can be similarly treated, and thus all forces acting through the plane Y Z will be included in the discussion.

To realize the action of A on the mass beyond the plane Y Z, construct with A, as a centre, consecutive spherical surfaces between atoms 1, 2, 3, 4, &c. to infinity. The quantity of matter in these hollow shells increases as the square of the radius: hence the aggregate force exerted by and on a shell involves a particular function of the square of the distance. The result already found for the action of A on the line of atoms 1, 2, 3, 4, &c., must therefore be multiplied by this function of x^2 to obtain the action of A on the series of spherical shells. For the gravitation branch of the curve the direct and inverse functions of x^2 will neutralize each other, and the curve will become a straight line parallel to the axis of X, giving an infinite aggregate attraction for an infinite medium. In other words, all gravitating shells give equal total attractions. For the atoms B, C, D, &c. nearly the same result will be found, by a like process, the only difference being in the cutting off portions from the ori-

gin end of the curve. The total action of A, B, C, D, &c., as of all the parallel atomic columns, will thus be to give this measureless preponderance to the attractions. Hence, a medium composed of atoms acting on each other according to the Boscovich force curve, would be unalterably cohesive, and the effects of the various attraction and repulsion branches interpolated between gravitation and the final repulsion would be totally insignificant. In an indefinite medium, the gravitation area would greatly exceed the area of the final repulsion, so that such a medium would of its own accord rush in upon itself, and become as it were but a single gigantic atom with a definite atmosphere surrounding it.

Whoever will follow this simple exposition of the effects of Boscovich's exponential or experimental curve of interatomic forces, when such atoms constitute a medium or mass, which case alone exists in nature, will see that this curve and the theory it expounds are deplorably false to facts, and destructive to their own pretensions. An exponential curve might doubtless be devised which would provide for the function of the square of the distance, and which would obviate the present gross exaggeration of attraction; though such an empirical curve seems little worth the labor. Surely however it is time for all to discard that misshapen and self-destroying exponential curve which has too long passed unchallenged, because Boscovich was a really great mathematician: great enough indeed, to have fully recognized the faults of his theory had they been actually pointed out to him. It is a strange oversight on his part, that he did not perceive the fallacy involved in his process of first constructing four atoms into a particle, four particles into a particle of the second order, &c.; as if his primary forces would recognize the ideal boundaries of such particles. He in fact neglects all actions except those between adjacent particles, when he passes to a medium, and Robison most pointedly does the same in his favorite conception of springs uniting atoms. If this neglect were really meant, the question would arise as to what becomes of the machinery for gravitation, and what springs are those binding the sun and earth? I cannot but regard the processes of reasoning employed by Boscovich and Robison as singularly rude and gross for such men, and such a subject; nor can molecular mechanics receive a more needful service than by the expurgation of views so abounding in error, and so obstructing the pathway to light. A fabric of objections and difficulties will surely arise in the mind of any well furnished investigator who will really think strictly on this renowned theory of spheres of force. The objections now presented are but specimens.

The speculation of Faraday lacks the definiteness of Boscovich's theory, and is not pushed into the field of molecular aggregation, nor indeed could it be with much hope of success. Ray

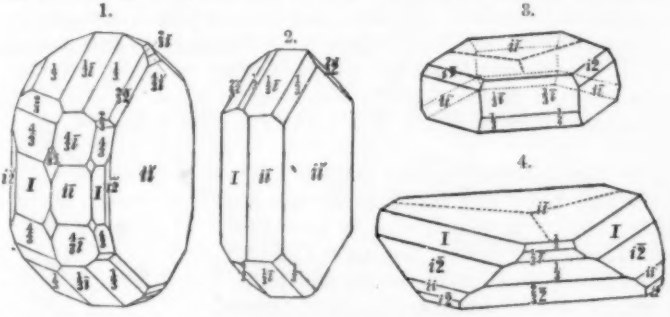
vibrations would be a very ingenious mechanism for gravitation, if the Newtonian law could be deduced in a tolerably simple manner from it, but this requisite seems to throw us back on the strange theory of a universal coexistence of all matter. Its inaptness for illustrating molecular mechanics is peculiarly striking if we attempt to imagine ray vibrations for the several phases of molecular constitution. In fact, the reduction of all forces to one law, such as that of Boscovich or Faraday, is like describing all animals as of the color of a chameleon.

In strange contrast with the Theoria and the Speculation, is the investigation by Mossotti, which is based on real mechanical principles, and which, though quite imperfect, leads to real results. By assigning definite size to atoms, and applying the simple Newtonian law of force to two kinds of matter, conditioned as in the Franklinian electrical theory, modified by Epinus, Mossotti has avoided most of the objections urged against the theory of spheres of force, and has given a glimpse at least of what heat is in the constitution of masses. By extending his investigation, and by supplying some deficient elements, molecular mechanics may at last be established on that simple and sure basis of ordinary mechanical principles, which Newton and Laplace have so distinctly foreshadowed, and which the expanding realms of physical science demand with a positiveness hitherto unknown.

ART. XXXI.—*Contributions to Mineralogy;* by JAMES D. DANA.

1. *On the relation of Leadhillite in crystallization, to the Anhydrous Sulphates and Carbonates.*

THE sulphato-carbonate, Leadhillite, shown to be trimetric in crystallization by Brooke and Miller, has three prominent points of interest: its approximation in form, viewed in one direction, to a regular hexagonal prism—its hemihedrism, which gives it a mono-



clinic aspect—its twin-composition, under which it takes a rhombohedral character. Figure 1 represents the known planes, taken

from a figure in Mohs's Mineralogy, pl. 13, fig. 97, and rendered nearly holohedral by adding (though of reduced size) the wanting planes. In the occurring crystal, the right-hand I, $\frac{4}{3}$, $\frac{2}{3}$, $i\bar{2}$ and $\frac{8}{3}\bar{2}$ are absent. Figure 2 represents an actual crystal of simpler form; it has but one plane I; and one plane $\frac{8}{3}\bar{2}$ on either side is obsolete.

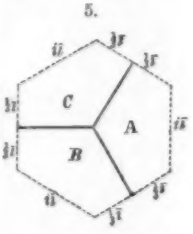
Figures 3 and 4 represent known twin forms, copied with altered lettering from tracings received by the author from R. P. Greg, Jr. The plane $i\bar{i}$ in these figures is made the base.

The prism $\frac{4}{3}\bar{1}$ has for its angle (at top) 120° 40′, which is very near the angle of a regular hexagon; so that these planes with the planes $i\bar{i}$ (fig. 1, 2) make up a hexagonal prism, varying 20 to 40 minutes from the angles of the regular hexagonal prism. And moreover as a consequence, the occurring planes between $i\bar{i}$ and $i\bar{i}$, and between $i\bar{i}$ and $\frac{4}{3}\bar{1}$, are nearly alike in angle—I and $\frac{8}{3}\bar{2}$ inclining towards $i\bar{i}$ at the same angle within 8′; and $i\bar{2}$ (not shown in the figures,—situated between I and $i\bar{i}$) and $\frac{4}{3}$, at the same angle within 6′. Again, the macrodome $1\bar{1}$, like $\frac{4}{3}\bar{1}$, is near 120° in its angles, giving 119° 40′ and 60° 20′, the acute edge of the dome in this case being above.

Brooke and Miller make the prism lettered $\frac{4}{3}\bar{1}$ the fundamental prism, and $i\bar{i}$ the basal plane. This gives simple expressions for the planes; but it does not appear to exhibit the true relations of the species. We arrive at this conclusion from the following considerations.

The twins consist evidently of *three* united crystals, as so regarded by Brooke and Miller, the plane $i\bar{i}$ (or planes in the series $i\bar{i}$, $i\bar{i}$, $i\bar{i}'$) forming three sides and only three out of the six (fig. 3). If the prism of 120° 40′ ($\frac{4}{3}\bar{1}$ above) be the fundamental prism, and analogous to that of Aragonite, the twins should be formed by composition parallel to the lateral planes of this prism, and moreover two of these lateral planes should be adjacent; or rather the perimeter would include at least four planes of the fundamental rhombic prism. But on the contrary, these planes are *alternate* instead of adjacent, and composition is not parallel to either of them, and hence there is no similarity to any known twin in the Aragonite group. This is seen in the annexed figure (fig. 5);—the sides of the hexagon lettered $\frac{4}{3}i$ are M of Brooke and Miller.

5.



The composition is in fact parallel not to $\frac{4}{3}\bar{1}$ (of figs. 1, 2), that is M of Brooke and Miller, but to $1\bar{1}$ which also is near 120°, having, as stated above, the angle 119° 40′. Hence this form $1\bar{1}$ ($i\bar{3}$ of Brooke and Miller) is better entitled from analogy and general principles, to be considered the fundamental prism of Leadhillite than $\frac{4}{3}\bar{1}$. In fact the prism $1\bar{1}$ (119° 40′) since it is the prism

parallel to which composition takes place, must be the true representative of the fundamental prism of Aragonite and the allied carbonates.

The relations of the sulphates and carbonates have been shown on page 53 of this volume, and had previously been brought out by Hausmann. It is there seen that the unit prism and domes of the sulphates and carbonates are as follows.

Barytes, a sulphate,	(1̆1̆) 116° 20′	(*I*) 78° 20′	(1̆1) 105° 24′
Anhydrite, "	" 118° 35′	" 77° 4′	" 107° 22′
Aragonite, a carbonate,	(*I*) 116° 10′	(1̆1̆) 81° 40′	(1̆1̆) 108° 26′

Adopting the prism of 119° 40′ in Leadhillite, as corresponding to the prism of 116° 10′ in Aragonite, as above shown, the corresponding angles are for

Leadhillite, . . .	119° 40′	76° 44′	107° 26′

For Barytes and Anhydrite (sulphates) the prisms of 78° 20′, 77° 4′ (the supplements of which angles are 101° 40′ and 102° 56′) are ordinarily taken as the vertical prisms; while in Aragonite, that of 116° 10′ is the vertical prism; the difference being one of position. The question now is, therefore, whether the prism of 119° 40′ is the true vertical prism of Leadhillite, and it is thence most closely related to the carbonates, or whether the vertical prism is that of 76° 44′ and 103° 16′, making its closest relation to the sulphates. The latter is the view adopted by the author in a former paper and in the lettering of the above figures, and it appears to be sustained by the following reasons.

1. The planes of the prism of 119° 40′ have not yet been observed, or if observed they are of very rare occurrence. In figure 1, the occurring planes are $\frac{1}{3}\tilde{1}$ and $\frac{4}{3}\tilde{1}$, without $1\tilde{1}$; and in the other zones, we find $\frac{1}{3}$, $\frac{2}{3}$, $\frac{4}{3}$, without the plane 1. Or, putting the crystal on $\tilde{1}\tilde{1}$ as its base, according to the view of Brooke and Miller, neither the prism of 119° 40′ is represented, nor any plane in that vertical zone. This is strikingly in contrast with the facts in the prismatic carbonates, in which the fundamental zone is represented by two or more planes in each of the species.

There is therefore a wide divergence from all the carbonates in this respect. And the divergence appears still wider when we consider that the most prominent zone is that of the prism of 120° 40′ ($\frac{1}{3}\tilde{1}$ in the lettering of figure 1).

2. The analogies between three trimorphous groups pointed out by the author in the last volume of this Journal, page 210, favor the view that the prism of 103° 16′ is the true vertical prism. As this point is important, the facts are here repeated.

Rhombohedral.	*Trimetric.*	*Monoclinic* (basal cleavage.)
Calcite (Ca, C) 105° 5′	Aragonite, Ca C, 116° 10′	Barytocalcite (Ca, Ba)C, 95° 8′
Dreelite (Ca, Ba) S; 93°–94°	Anglesite Pb S, 103° 38′ Anhydrite Ca S, 102° 56′ Barytes Ba S, 101° 40′	Glauberite (Na, Ca) S; 83°
Susannite, Pb S+3Pb C 94°	Leadhillite, Pb S+3Pb C 103° 16′	Lanarkite, Pb S+Pb C, 85° 48′

The rhombohedral and monoclinic forms, and the trimetric and rhombohedral, have nearly a common difference, 10 to 11 degrees; moreover the sulphates and carbonates in each column differ in angle by nearly a common difference, or 12 to 13 degrees. At the same time, in each, the sulphato-carbonates agree with the sulphates. The fundamental prism of aragonite has its angle 11 degrees larger than the rhombohedral angle of calcite; the prism of anhydrite 9 or 10 degrees larger than the rhombohedral angle of Dreelite; and therefore the prism of Leadhillite is that one which differs by some similar angle from Susannite. The parallelism above is so exact both vertically and horizontally, that the argument must be allowed to have much weight. Its authority becomes irresistible when viewed in a different light.

3. Dreelite is dimorphous with Anglesite and Barytes,—the same compound, essentially, occurring here under a *rhombohedral* and *trimetric* form. Moreover, Dreelite and Susannite are identical nearly in angle, or are homœomorphous; while Susannite and Leadhillite constitute a second case of dimorphism parallel with that just mentioned. Since now this sulphato-carbonate under its rhombohedral form (Susannite) is homœomorphous with the sulphate (Dreelite) and not with the carbonate, hence in its trimetric state (Leadhillite) it should be most closely related to the sulphate (Anglesite or Anhydrite). As the sulphuric acid dominates over the carbonic acid in the Susannite, one form of the sulphato-carbonate, it must also dominate over it in that same sulphato-carbonate under any other form it may present, or in Leadhillite; whence in either case, the forms should be homœomorphous with the corresponding sulphates.

It seems therefore to follow that 103° 16′ is the true vertical prism, as adopted in the figures.

4. We may find further objection to regarding Leadhillite as related most nearly to the prismatic carbonates in the fact that in this case, the most perfect cleavage would be basal, which is not true of any known species of the aragonite group. But after the arguments above stated, we hardly need look further for evidence. An objection to the view adopted might be suggested from the fact that through hemihedrism, one of the prismatic planes *I* is wanting. But there is no reason why hemihedrism should not result in suppressing one of these as well as other planes, and the objection is of little weight. Moreover the fact, which we learn from Brooke and Miller that the plane *I* is also a direction of twin composition, shows that this plane is at least one of prominent or fundamental value in the crystal.

Upon the view which has been discussed, the species *Anglesite, Anhydrite* and *Leadhillite* have the following dimensions:

	Prism $I : I$	Dome 1̄1	Dome 11̄	Axes $a : b : c$
Anglesite	103° 38′	62° 42′	75° 29′	1·6415 : 1 : 1·2715
Anhydrite	102° 56′	61° 25′	72° 38′	1·6836 : 1 : 1·2557
Leadhillite	103° 16′	60° 20′	72° 34′	1·7205 : 1 : 1·2632

We add a remark with regard to the rhombohedral character of the twins.

In the twins, like fig. 4, the series of planes in each sextant are closely related; thus, as has been observed, the planes I, $i\bar{2}$, $\tilde{i}\tilde{i}$, $\dot{i}\bar{2}$, respectively have nearly the same inclinations on $\tilde{i}\tilde{i}$ as $\frac{2}{3}\bar{2}$, $\frac{1}{3}$, $\frac{1}{3}\bar{7}$, $\frac{1}{3}$ have on the opposite $\tilde{i}\tilde{i}$. From I the planes narrow downward, and from $\frac{2}{3}\bar{2}$ they narrow upward and so alternating around. In fig. 3, there is a corresponding alternation, though of less extent. Comparing it with the simple crystal, it looks like an inversion of the alternate sectors; but as the compound form contains only three simple crystals, an actual alternate inversion is impossible. As the plane I is a fundamental plane, the occurring one (see fig. 2) should have a supremacy in the twin, and with it, the series to which it belongs: and as this series in the simple form diminishes from I to the opposite side (the right in fig. 2), this would imply a reverse enlargement of the next or $(\frac{1}{3})$ series, and by this alternation the rhombohedral character would result. The fact, moreover, that the compound is dimorphous and that the other form is rhombohedral, with the same angles nearly as the twin of Leadhillite (as shown by Brooke and Miller) may suggest further reason why the twin should take the alternating or rhombohedral character.

These views are especially interesting as bearing on the subject of dimorphism, and illustrating the passage of a trimetric form to a rhombohedral.

2. *On the so-called Silico-Titanates and Silico-Tantalates.*

In a former number of this Journal it was shown that Sphene was a true silicate of the form $(\dot{R})^3\,\ddot{S}i^2$, or what is equivalent $(\dot{R})\,\ddot{S}i^{\frac{2}{3}}$, in which

$$\dot{R}\,(\text{or } R^2O^2) = Ti\,O^2 + Ca\,O.$$

It is consequently *trimorphous with Andalusite and Kyanite.*

In this volume, page 130, the author also observes that the formula of Keilhauite, on the same principle may be

$$(\dot{R}^3, \dot{R})\,\ddot{S}i^{\frac{3}{2}},$$

and that of Wöhlerite

$$(\dot{R}^3, Zr, Ni)\,\ddot{S}i.$$

The special formula of this last afforded by the analyses, is

$$(\tfrac{6}{10}\dot{R}^3 + \tfrac{3}{10}Zr + \tfrac{1}{10}Ni)\,\ddot{S}i, \text{ or } 6\dot{R}^3\,\ddot{S}i + 3Zr\,\ddot{S}i + Ni\,\ddot{S}i = (\dot{R}^3, \dot{R}, \dot{R})\,\ddot{S}i.$$

The analysis by H. Rose of *Tscheffkinite* (Pogg. lxii, 591) appears to lead to the same general formula with that of Keilhauite, or

$$(\dot{R}^3, \dot{R})\,\ddot{S}i^{\frac{3}{2}}.$$

Schorlomite afforded Whitney the formula—

$$Ca^3\,\ddot{S}i + Fe\,\ddot{S}i + Ca\,\ddot{T}i^2$$

Making the $\ddot{T}i$ a base as above, the oxygen ratio for the bases and silica is 6 : 11. But if the silica as obtained be a little too high,

the ratio may be $5\frac{1}{2} : 11$ or $1 : 2$, whence would come the formula

$$2\dot{R}^3\,\ddot{S}i^{\frac{1}{2}} + \dot{R}\,\ddot{S}i^{\frac{1}{2}} \quad \text{or } (\tfrac{2}{3}\dot{R}^3 + \tfrac{1}{3}\dot{R})\,\ddot{S}i^{\frac{1}{2}} \quad = (\dot{R}^3, \dot{R})\,\ddot{S}i^{\frac{1}{2}}$$

(analogous to that of staurotide) = Silica 23·3, titanic acid 22·9, peroxyd of iron 22·4, lime 31·4 = 100. It gives 2 per cent. too little of silica, according to the analyses, while agreeing closely with the results in other respects.

Mosandrite, in a similar manner, gives for the oxygen ratio of the protoxyds, peroxyds and silica 1 : 2 : 3, or of bases and silica 1 : 1 (precisely 16·57 : 15·86); affording the formula

$$\dot{R}^3\,\ddot{S}i + 2\dot{R}\,\ddot{S}i + 4\tfrac{1}{2}\dot{H} \text{ or } (\tfrac{1}{3}\dot{R}^3 + \tfrac{2}{3}\dot{R})\,\ddot{S}i + 1\tfrac{1}{2}\dot{H}, = (\dot{R}^3, \dot{R})\,\ddot{S}i + Aq,$$

which, excluding the water, is the formula of epidote. The crystallization of mosandrite has not been clearly made out.

It is probable, that there are no true silico-titanates or silico-tantalates, the titanic or tantalic acid being a base in each case.

3. *Tourmaline.*

The author has shown that the general formula of Tourmaline is

$$(\dot{R}^3, \dot{R}, \ddot{B})\,\ddot{S}i^{\frac{4}{3}},$$

the oxygen ratio between the silica and all the other bases being 3 : 4, as ascertained by Rammelsberg, and this being the only constant ratio. The oxygen ratio for the protoxyds, peroxyds, and boracic acid, as deduced from Rammelsberg's analyses, varies greatly. Group I, affords mostly the ratio 4 : 12 : 4,—Group II, the ratio 4 : 15 : 5,—Group III, the ratios 4 : 21 : 6, 4 : 24 : 7, etc. —Group IV, the ratios 4 : 36 : 11, 4 : 40 : 12, etc.—Group V, the ratios 4 : 48 : 13, 4 : 56 : 12, etc.*

For Group I, the special formula is hence

$$\dot{R}^3\,\ddot{S}i^{\frac{4}{3}} + 3\dot{R}\,\ddot{S}i^{\frac{4}{3}} + \ddot{B}\,\ddot{S}i^{\frac{4}{3}} \quad = (\tfrac{1}{5}\dot{R}^3 + \tfrac{3}{5}\dot{R} + \tfrac{1}{5}\ddot{B})\,\ddot{S}i^{\frac{4}{3}}.$$

For the Red Tourmaline of Elba, in Group V,

$$\dot{R}^3\,\ddot{S}i^{\frac{4}{3}} + 14\dot{R}\,\ddot{S}i^{\frac{4}{3}} + 3\ddot{B}\,\ddot{S}i^{\frac{4}{3}} \quad = (\tfrac{1}{17}\dot{R}^3 + \tfrac{14}{17}\dot{R} + \tfrac{3}{17}\ddot{B})\,\ddot{S}i^{\frac{4}{3}}.$$

These appear to be the extreme variations in the species Tourmaline, if we exclude the analysis (No. 30) of a somewhat decomposed variety from Rozena. The formulas for the other ratios may be easily written in like manner, in accordance with the general formula above.

Axinite has in like manner the general formula

$$(\dot{R}^3, \dot{R}, \ddot{B})\,\ddot{S}i;$$

the analyses afford, as the special formula under this type

$$\dot{R}^3\,\ddot{S}i + 2\dot{R}\,\ddot{S}i + \tfrac{1}{2}\ddot{B}\,\ddot{S}i,$$

and this formula was suggested by Rammelsberg in his Handwörterbuch, i, 72.

* The oxygen ratios deduced by Rammelsberg for the protoxyds, peroxyds and silica, *the boracic acid* being included with the silica, are for Group I, 1 : 3 : 5; II, 1 : 4 : 6; III, 1 : 6 : 8; IV, 1 : 9 : 12; V, 1 : 12 : 15.

ART. XXXII.—*Notice of the Life and Writings of the late Dr. Waldo Irving Burnett;* read before the Boston Society of Natural History, July 19th, 1854, in accordance with a vote passed at the previous meeeting. By JEFFRIES WYMAN, M.D.

Mr. President—From time to time Death has entered our circle, and taken from our number one and another of those who have been our most active associates, and to whom we have been bound by the ties of personal regard or of friendship. In nearly every instance they have been removed in full manhood, or even at a later period, when the labors of a life of the ordinary length had been nearly finished. But never before has there been taken from amongst us one who, in his devotion to natural science, has, in so brief a life, left so many memorials of zeal and industry as he, to whose memory we would now pay our tribute of respect.

WALDO IRVING BURNETT was born in the town of Southboro', Mass., July 12th, 1828. His father (the late Dr. Joel Burnett) was a man of distinguished excellence in his profession, and to the qualities of a good and useful citizen united those of an ardent lover of nature, of whose works he was a close and faithful observer. Botany and Entomology especially received his attention, and without the aid of genial spirits, or the intercourse with kindred minds, were studied with no ordinary zeal during the few leisure moments which were left him after the demands upon his time by a laborious profession had been satisfied. His love of nature was transmitted to the son, and was manifest in early boyhood, when the observation and study of insect life took a strong hold upon his mind. His father experienced a just pride in witnessing these tendencies; but in place of encouragement, which he at first extended with delight, he was soon, though reluctantly, obliged to substitute restraint. His son's mind was too intently absorbed in his pursuits, and fears were excited lest his studies, prolonged into hours stolen from the usual period of repose, should be attended with disastrous results to his physical constitution. His passion, however, grew with his growth and strengthened with his strength, and in the face of all obstacles, through health and through sickness, from an early youth to his early grave, it was never abated.

He had not the advantage of a collegiate education; this he chose to forego, not from any indifference to its value, but from a sensitive unwillingness to subject his father to any unnecessary expenditure of his means. He gave early indications of great mental activity, and mastered with ease all the studies of the Academy; in mathematics, especially, he was unusually proficient, and drew from his teacher the confession that in this de-

partment he was no longer capable of giving him instruction; and it was the habit of other teachers in the neighborhood to send to young Burnett for the solution of difficult questions which they themselves were incompetent to master. Almost without assistance, at a later period, he made himself familiar with the French, Spanish, and German languages, and during the latter part of his life had made some progress in the Swedish.

At the age of sixteen he had become thoughtful beyond his years; and then commenced the development of those tendencies in his mind which ever afterwards were so conspicuous, and which continued to exert a controlling influence, viz.: the desire of gaining an insight into the nature of things, and of forming philosophical ideas and conceptions of natural processes, conceptions and ideas which can be obtained only by the exercise of the higher powers of the mind. Mesmerism, materialism, and theological questions occupied his thoughts, and were frequently written upon and discussed by him. On all of these he manifested independence and continuity of thought, and persistence in whatever direction his mind was turned. It was at this early age that his interest in the study of medicine commenced, when he accompanied his father in his professional visits, and witnessed the effects of disease, as manifested in the examination of bodies after death. Entomology now especially engrossed his thoughts, and nearly all his leisure moments were occupied in collecting, studying and classifying insects. While yet in his sixteenth year his father died. This event materially changed his prospects, and was met with firmness and decision, and in the course of the following year, finding that something must be done for his support, he commenced teaching school, and at the same time gave his attention to the study of medicine.

The subsequent years of his student life were spent under the direction of Dr. Joseph Sargent, of Worcester, with whom there grew up warm mutual personal regard and friendship: in the Tremont Medical School in Boston, which has given to the profession so many zealous and productive laborers in medical science: and in the Massachusetts General Hospital. He was ardent and industrious as a medical student, but never allowed his attention to be withdrawn from the study of nature, the microscope becoming his constant companion, and a source of never-failing pleasure. As evidence of his ability it may be stated that in two successive years he gained the annual prize offered by the Boylston Medical Society. The subject of the first essay was *Cancer*, treating especially of its microscopic structure; and of the second, *The Sexual System, or the production of being, considered as to its physiology and philosophy.*

In 1849, at the age of 21, he graduated in medicine, and soon after visited Europe, where his attention, especially at Paris, was

given almost exclusively to natural history and microscopic observation. The expectations of intellectual progress which he now looked forward to with so much interest, were soon doomed to severe disappointment. It was in Paris that he received the first serious warning that consumption, the disease which eventually destroyed his life, had already marked him for its early victim. After an absence of only four months, he re-embarked for America, to receive the benefit of a more genial climate in one of the southern states, and each successive winter he passed either in Carolina, Georgia or Florida, in order to avoid the inclement and uncongenial climate of New England. He had now no permanent location, was constantly shifting from place to place, to mitigate, as far as possible, the steady progress of his disease. Everything seemed adverse to anything like connected study. Nevertheless, it was during these few unsettled years that he accomplished an almost incredible amount of intellectual labor. He was incessantly occupied with his microscope; his mind was ever on the alert, and he allowed scarce a day to pass without some observation, without something added to his stock of acquired knowledge.

In the winter of 1851 he delivered at the Medical College in Augusta, Ga., a successful course of lectures on Microscopic Anatomy. In the summer of 1852 he prepared the principal work of his life, the Essay which received the prize from the American Medical Association. His two former prizes were competed for only by his fellow students; but the third, it is no small praise to say, was open to the competition of the whole medical profession throughout the country.

While yet a medical student he became an active member of the Boston Society of Natural History, and was soon after elected Curator of Entomology. In 1851 he was elected a member of the American Academy of Arts and Sciences—one of the youngest members ever admitted into that body. His communications to different scientific bodies and journals were very numerous and on a great variety of subjects, and give such evidence of industry and enthusiasm as cannot fail to excite our wonder and admiration. They are too numerous for analysis or even enumeration in this place; but some of the more important ones are found under the following list of subjects, which comprises those of about one-third of the whole number of his memoirs and communications, and which serves to show that his mind was interested in a great variety of questions, and that whenever an opportunity for investigation presented itself, he was always ready with a cheerful heart and patient industry to enter upon his work.*

* His various scientific papers or abstracts of them may be found in the *Proceedings*, also in the *Journal of the Boston Society of Natural History*. In the *Proceedings of the Boston Society for Medical Improvement*, in the *Proceedings* and in

"*On the Hybernation of Insects, and its Relation to their Metamorphosis.*"

"*An account of certain microscopic animals found in a person who died of an enlarged spleen.*"

"*On the external parasites of warm-blooded animals.*" This was a subject to which he had devoted much attention, and in illustration of which he had made large collections of specimens preserved for microscopic study.

"*On the embryology of the Articulata,*" including remarks on the alternation of generations in the Humble bee, (Bombus Americanus,) in which last he ascertained that three generations are produced from one impregnation.

"*On the luminous spots of the great Fire Fly of Cuba.*"

"*Observations on the seventeen-year locust.*"

"*On Spermatozoa.*"

"*On the origin, development and structure of the kidneys throughout the vertebrated division of animals.*"

"*Notes on the Rattle-snake, relating to its dentition, to the physiological effects of its poison, and to alcohol as a remedy.*"

"*Some account of an Insect,* (*Rhinosia pomatella,* Harris,) *and its recent injuries to the fruit and forest trees of New England.*"

"*On the development of Viviparous Aphides, or plant lice.*" This is a subject of great interest, and it was investigated with great ability. Since the days of Bonnet it has been well known that several successive generations of Aphides are produced after a single impregnation. Dr. Burnett studied the successive generations as they first appear in the body of the parent, as illustrated by the species infesting the hickory. If a fully developed, but wingless Aphis is examined in the spring, it is found to contain an embryo nearly mature; and this embryo contains already the first germs of the third generation, in the form of single cells or a small number of cells enclosed in a sac. While a few germs are thus formed, others are formed by their subdivision from constriction, until the requisite number is obtained. When they have reached the size of about one three-hundredth of an inch, a yellowish mass forms at one extremity of the egg, and then commences the development of the parts of the insect, which eventually enclose the mass just mentioned. It is this last yellowish mass which furnishes the materials for the next generation. All this, it should be remembered, is effected without the aid of any distinct reproductive organs. There is no ovary or oviduct, but the embryos are developed in the cavity of the abdomen, and discharged through a genital opening merely. In view of the fact that the Viviparous aphides are sexless, Dr. Burnett

the *Memoirs of the American Academy of Arts and Sciences,* in the *American Journal of Science,* in the *Boston Medical and Surgical Journal,* and in the *American Journal of Medical Science.*

regards their mode of reproduction as belonging to the gemmiparous type. Viewed in this way, the different broods cannot be looked upon as so many generations; but on the contrary, the whole suite, from the first to the last, that is, till the production of a winged Aphis, constitute but a single generation. This explanation by a species of budding seems far more satisfactory than that which supposes that either cells or nuclei of the first individual are transmitted by successive inclusions to the last. As this latter idea cannot be supposed to be the result of direct observation, and as no proof is adduced that identical cells and nuclei really pass from one generation to the other, the whole stands merely as an ingenious theory, while Dr. Burnett's explanation [and this view is not proposed for the first time by him,] is in accordance with direct observation. But, in accepting his view, we are compelled to admit the hypothesis, that the germinating force imparted to the first ova is transmitted to the successive broods without the aid of spermatozoa.

"*On the microscopic appearances presented in the intestinal discharges and muscular fibres of a patient who died of the epidemic cholera.*"

"*Tissue and its retrograde metamorphosis.*"

"*On the Geology and other points connected with the natural history of Florida.*"

"*Considerations on a change of climate by northern invalids, and on the climate of Aikin, S. C.*"

"*Considerations of some of the relations of climate to tubercular disease.*"

To these should be added his various critical notices of recent scientific publications in Silliman's Journal, which in view of the short time he occupied the position of associate editor, were quite voluminous, and serve to give us a good idea of his powers of analysis and discussion.

There is no one of his productions, which embodies more of the results of his labors, than the prize dissertation, consisting of two hundred closely printed octavo pages, presented to the American Medical Association in the year 1851, and entitled "*The Cell, its physiology, pathology and philosophy, as deduced from original observations; to which is added its history and criticism. 'Natura in minimis maxima est.'*"

To those who are acquainted with modern physiology, it will be seen at once that he had selected a great subject, one which even the most accomplished minds might approach with distrust. The nucleated cell! that minute organic structure which the unaided eye cannot discern, yet constituting the first stage of every living being, the seat of so many of the complex phenomena of animal and organic life, and the agent by which even the mind itself retains its grasp and exerts its influence upon the living

structures with which it is associated. In entering upon so difficult a subject as this, it was not expected, nor is there any reason to suppose that he himself expected, that he should not lay himself open to criticism. The ablest living histologist, Kölliker, in speaking of the subject of the development of tissue, uses the following language: "Not only does histology not possess a single law, but the materials at hand from which such could be deduced are as yet relatively so scanty, that not even any considerable number of general propositions appear well founded." As laws and general propositions were among the especial objects of Dr. Burnett's researches, it will be seen at once that he has entered boldly into a contested field. But it is to follow him in his labors, and not to hold up to criticism his results, that we have at present to do.

His subject is discussed under the following heads:

1st. *Cell-genesis*, under which he treats of the origin of cells, and advocates a peculiar mode of development, which he claims as original with himself, and the result of his own observations.

2d. *Cell physiology*, or healthy function.

3d. *Cell pathology*, or diseased function.

4th. *Cell philosophy*, or 1st, the relations of cells to the teleological view of organization; 2d, the direct agency of cells in the production and manifestation of nervous power, the intellectual processes, &c.

The general results of his studies of cell life and cell genesis are in his own words as follows: "The great outstanding fact which appears before us as the result of these studies is, that there is fundamental unity of organization. This we have seen to consist in elementary particles, which in both animals and plants are formed upon a common plan. It was the opinion of Schwann and Schleiden, who truly originated this view, that this plan consisted in the preëxistence of a solid fundamental body, (the nucleus) around which is formed a membrane ultimately expanding and constituting the cell. It has been one of my objects to show, that this is not of universal application, by an attempt to demonstrate another mode of cell formation, which is that the fundamental idea of a cell is a simple vesicle, and that the nucleated cell is simply one cell containing another within its walls. With Schwann the nucleus is *exogenous* and *germinative*—with me the nucleus is *endogenous* and reproductive.

"The two conclusions of the studies of cell life are then 1st. The existence of an elementary particle, having an invariable unity of expression, *the cell.* 2d. The universality of the application of this particle for the formation of organized parts, *the tissues.*"

In studying cells in relation to pathology, he regards this last as an erring physiology, and concludes, that, both as to their gen-

esis and general aspect as cells, those which belong to abnormal cannot be distinguished from those belonging to normal conditions of life. The genetic and general relations of cells in physiology and pathology are therefore the same. Their difference does not relate to structure, but to their destiny. Physiological cells must be considered teleologically, but pathological ones have no ulterior object.

Each of the different heads of his dissertation he discusses with great ability, and gives ample evidence that he is not only familiar with the scientific labors of others, but that he is perfectly at home in the different departments of investigation which his essay involves. If it be allowable to express an opinion of its merits in general terms, it may be truly said that it gives evidence of wonderful zeal and industry in research, of acute powers of observation, and of great readiness in perceiving general relationships. It is in connection with this latter faculty that he seems the most liable to error. He appears to have partaken something of the spirit of Oken, and to have given way at times to the suggestions of the imagination, instead of subjecting himself to the severe mandates of reason, and the rigid rules of induction. This is naturally the fault of youth, and for which scientific minds, at the present time, with their tendencies to hasty generalization, may be justly said to be in part responsible. But in one who combined industry, a desire for truth, and an almost unlimited patience in observation, it might have been fairly anticipated that, sooner or later, the better and safer qualities of the mind would have eclipsed all others.

While constantly active as an observer, Dr. Burnett found time to engage in another service which occupied some of the latest hours of his life, and the non-completion of which was a source of anxiety to him in his last moments. This was the translation from the German, of the Comparative Anatomy of Siebold and Stannius. All who are familiar with the published volume, will not fail to see in it another proof of his industrious habits as exhibited not in the translation merely, (itself in this case no ordinary labor) but in the numerous additions to science which, scattered far and wide through scientific journals, have been brought together, and in the contributions he himself has made from his own stores of accumulated observations.

The last scientific investigation to which his time was devoted was into the natural history of the *Orange insect*, which is so destructive to the orange trees of Florida. The habits of this insect he had studied during his last winter's residence in Florida, and had prepared a memoir in reference to it for the American Association for the Advancement of Science, but his ill health prevented his attending their recent meeting.

Such is an imperfect sketch of the scientific labors of our late associate. It only remains to consider his life from another point of view, in regard to its moral aspect. Of this I do not feel justified in treating at length, as my relations to him were not sufficiently intimate to speak from personal observation; but from all I can learn from his associates, from his fellow-students and his more intimate friends, he was a kind and affectionate son and brother, one who enjoyed to an unusual degree home and all its associations; he was a man of a truly benevolent heart, into which irreverent thoughts seemed to gain no admission, or from which they certainly obtained no expression. In all of his studies of nature he seems to have had a pervading perception of God in his works, and often in eloquent words gives expression to his feelings, when some new manifestation of divine wisdom was uncovered to his inquiring mind.

Dr. Burnett's zeal and devotion could not fail to awaken a warm interest wherever he went, among those with whom he associated. He became acquainted with the leading naturalists of the country, and obtained from them and others, willing aid and counsel, as well as respect for his great acquirements. To them he always felt warm feelings of gratitude. But there was one, to whom, more than all others, he was especially grateful, a friend and relative, who at an early period, perceived the indications of uncommon promise for the future, and who with kind heart and benevolent purpose aided and encouraged him in all his undertakings.

He had religious faith and religious hope. To a speculative mind like his, it seemed almost a matter of necessity that the momentous questions which the problem of life involved, should sooner or later, have been presented for examination and discussion, and that before any settled convictions could be reached, they should have found him perplexed and in doubt. Doubts and perplexities in his mind did exist, but eventually they gave way and were replaced by faith and hope, which lightened his burden when, weary and exhausted, he approached the end of life. He had been long accustomed to look upon death and to talk about it as an event that he must meet at an early period. But death, if not imminent, is something that all look forward to calmly and without emotion, and when we speak of it we are not sure that we give utterance to our most solemn feelings and convictions. But there is one moment when, if ever on earth, the heart, if it opens itself, does so without disguise, if it give utterance, does so without reserve; it is that dread moment when death approaches so near that there is no alternative but to look upon earthly life as finished, its account made up, and when all that remains for the mind to dwell upon, is the dissolution of the body and the realization of another life. A few days before he

died our late associate returned after a winter's absence, to the home of his family, his bodily health exhausted, his energies prostrate. At first he entertained the hope that as before, rest and quiet might restore him partially at least to his usual health, and that he might have yet another opportunity of continuing those labors which he so fondly cherished; but his fast declining strength, the anxiety of those around him, the announcement of his physician and his own quick perceptions soon told that life was drawing to a close, and that for him the great moment was near. In all this he was calm and serene, conversed on the approaching separation without faltering, gave utterance to expressions of deep affection to those who were bound to him by the ties of kin, uttered his prayer for forgiveness, and expressed the solemn conviction, which now rose paramount to every other, that if there yet remained much for him to live for, there was yet far more to die for. On Saturday morning, July 1st, a few days before the completion of his twenty-sixth year, he died.

We cannot but sensibly feel, that in his death we have lost an associate of no ordinaty talents; we can point to no other member of our Society, and to not more than one other naturalist in our country, who has given such proofs of zeal and industry, and who, in so short a life, has accomplished so large an amount of scientific labor. Had he been spared to future years, we cannot but feel the assurance that he would have acquired for himself a far higher place and a still more honorable name in the annals of science. Let us cherish his memory and profit by his example.

The Resolutions which follow, prepared at the request of the Society by Prof. Wyman, were unanimously adopted:

Resolved, That the members of the Boston Society of Natural History have learned with deep regret the death of Dr. Waldo Irving Burnett; that, in his decease, we have lost a most active and zealous associate, and science an ardent, disinterested and productive laborer.

Resolved, That to the family of our late associate, we would offer our deep sympathy for their affliction, in the loss which they have sustained by the early death of one, with whose memory is associated so much of honorable devotion and noble self-sacrifice.

On motion of Dr. Abbott, it was voted, that Dr. Wyman be requested to prepare a copy of the Notice and Resolutions for publication in Silliman's Journal.

[It is with deep sorrow that in place of the usual Contribution from Dr. Burnett for this Journal, we have to present to our readers his obituary. One of the most earnest, faithful and profound laborers in science in the country has ceased from his work while

yet in the midst of research and with new truths constantly developing before his scrutinizing eye. During his connection with this Journal, he exhibited a deep interest in the progress of his special departments, gathering from every available source the means of enriching his papers; moreover he brought to bear upon the subjects before him a large amount of original research which enabled him to select the truth from error and pronounce judgment on the observations of the best investigators. Dr. Burnett was among the few in the land who not only knew well the latest results of the studies abroad in his department, but also labored successfully in testing those results, and more than this, contributed directly to the further progress of science.

The just tribute to the memory of our friend and colaborer by Dr. Wyman, renders it unnecessary for us to indulge in further remarks. His death is a grief to his friends; but science has even more cause to mourn.—Eds.]

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On the continuity and force of the current of the magneto-electric machine.*—Sinsteden has published an interesting paper on this subject from which we extract those portions which appear of most importance in a practical point of view. The author's experiments were made with special reference to the employment of the magneto-electric machine in telegraphic operations, but it is clear that they are of equal importance for another practical application of the instrument, namely, to the process of electrotyping. When the inductor of a large machine was made to revolve thirty times in a second so that the iron cores approached and receded sixty times from the poles of the magnet, 120 single current impulses were of course produced, separated by as many intervals of about equal length. A single interval lasted therefore about the $\frac{1}{240}$ of a second, and as an experienced telegraphist with the key of Morse's apparatus opens and closes the circuit about twelve times in a second, there are in this case on the average twenty current impulses for every closure of the circuit. This is quite sufficient to work Morse's apparatus even upon very long lines; a preliminary experiment made by the Royal Telegraph Bureau in Berlin shewed that on the line of 74 (German) miles from Berlin to Dantzic, Morse's registers were worked with uniformity and certainty even when registers were placed at many intervening stations. During a long correspondence not a single uncertainty or error occurred while the magneto-electric machine was used, and the needle of a galvanometer at Dantzic gave a deviation several degrees greater than with the zinc-carbon battery usually employed. Other experiments showed that the machine was capable of transmitting signals with perfect regularity and certainty to a distance of 100 German (about 400 English) miles. In order to

demonstrate still further the continuity of the current, the author examined its magnetic and physiological effects. Two electro-magnets introduced into the circuit supported together 750 lbs., and a vibrating hammer apparatus gave regular and very rapid beats but no musical tone. When the intermitting spring of the apparatus was removed, the conductors held in the hands wet with salt water, and the inductors made to revolve three times in a second, strong shocks were felt in the arms which however were not sharp and sudden like those produced when the intermitting spring was used, but rose, swelled, and sank gently, so that they were received like waves and consequently easily borne. When the entire force of the steel magnet was diminished by laying on armatures and the rate of rotation increased till the inductor made thirty revolutions in a second, no shocks were felt, but instead, a very painful lasting contraction, first in the arms and then in the throat. These phenomena are analogous to those which are produced by a battery of 100 or 150 pairs, though of course it is not for an instant to be supposed that the current of the machine is as continuous as that of the battery. The result is the same, as Neeff and Pouillet have shown, when a battery and an interrupting wheel (blitz rad) are employed. From these experiments the author concludes that the current of the magneto-electric machine possesses sufficient continuity and constancy to be used in telegraphing, and that it approximates to the degree of constancy of a galvanic battery of many pairs. With respect to the intensity of the current of the magneto-electric machine and to certain peculiarities which give it in some measure a *specific* character, the author's experiments are particularly interesting. When the uninterrupted current from an inductor with free helices was conducted through a platinum wire ten inches long and one-tenth of a line in thickness, eight inches of the wire were ignited to whiteness: when a platinum wire one millimeter in thickness was employed as the interrupting spring, dense solid white sparks two lines thick and nearly four lines long were produced, in which the point of the platinum wire was ignited and fused. When a steel watch spring was employed in place of the platinum wire, one-fourth of inch of the steel was ignited and burnt off and a piece of the watch-spring a foot long could be fused in less than a minute. In this experiment the steel spring should form the positive and the interrupting wheel the negative pole; in this way the spring burns most brilliantly and the iron wheel is not injured. When the current was conducted through a voltameter having bright platinum plates immersed in dilute sulphuric acid, about three cubic inches of gas were evolved in a minute. After the plates had been used a long time, they had entirely lost their metallic lustre and become dull and dark gray, while upon the bottom of the voltameter a fine black powder had collected. Both the platinum plates and the black powder, possessed in a high degree the property of causing oxygen and hydrogen to unite: the powder proved to be metallic platinum while the microscope showed that the surface of the plates was rough as if eaten into. With two voltameters five cubic inches of gas were obtained per minute; with six voltameters in the circuit, having however plates of different dimensions, 8·8 cubic inches of the mixed gases were produced. In this last experiment it was very remarkable that

no strong polarization of the platinum electrodes of the six voltameters took place, since on uniting them by a delicate galvanometer, the needle shewed only a very feeble secondary current. Polarization of the electrodes in a very remarkable degree was however produced when the magneto-electric current was passed for a short time through decomposing cells in which the electrodes were lead, silver or nickel in dilute sulphuric acid, or zinc in solution of caustic potash. Two cells containing very dilute sulphuric acid and electrodes of pure silver 3 inches long and $1\frac{5}{8}$ inches wide, were introduced into the circuit. After the current had passed, during a few seconds, the silver plates became covered with gas, the positive plates then became blackish gray, the negative plates covered with a delicate gray coating which rapidly passed to velvet black, increased in thickness, and finally dropped from the plates becoming at the instant whitish gray. The same change of color took place every time that the current ceased, the coating again becoming black when the current again began to pass. When the plates had stood a long time without the current passing, both negative and positive plates were covered with a thick yellowish white coating, and the precipitate had the same color. Spread out on paper this precipitate shews the whitest silvery lustre and proves to be finely divided metallic silver. If after the current had passed half a minute the electrodes were removed from the machine and used as a battery, sparks could be obtained, water decomposed and iron and platinum wire one inch long ignited and fused. When this battery was closed by the spiral of the magneto-electric machine—of course at rest—and by the body, severe shocks were received every time the current was broken: brilliant and loud sparks could also be obtained by this arrangement. The polarization of the battery lasts fifteen minutes in tolerably uniform intensity, and the battery gives, when closed only four or five times in a minute and then immediately opened again, during this whole time equally strong sparks and shocks. When a solution of caustic potash is added to the dilute sulphuric acid in which the plates are dipped the result is very different; no secondary current is produced but there is a strong evolution of *ozone*. Plates of lead in dilute sulphuric acid gave as strong and lasting a secondary current as plates of silver. In this case the positive electrode became covered with a dense layer of peroxyd of lead; the negative electrode became blackish gray but no precipitate was deposited upon it; no ozone was evolved during the passage of the current. The addition of a solution of caustic potash to the sulphuric acid produces a strong evolution of gas and of ozone, but in this case also there is no secondary current. Plates of nickel gave a strong and lasting secondary current. Plates of zinc in a solution of caustic potash give off no ozone; the secondary current is like that of the other metals, but the decomposing cell is soon rendered useless by a heavy white precipitate.

[*Note.*—It is to be hoped that the value of the magneto-electric machine in telegraphing operations will receive in this country the attention which is certainly due to it.—W. G.]—*Pogg. Ann.*, xcii, 1, *May*, 1854.

2. *Galvanic reduction of metallic chromium.*—BUNSEN has communicated the results of some further investigations in electrolysis and has shown how chromium, manganese, and several other metals may read-

ily be reduced in small quantities from solutions of their chlorids. The author in the first place has found that the *density* of the current exerts a most important influence on the nature of the products of the electrolytic action; by the *density* of the current is understood the intensity divided by the surface at which the electrolysis takes place. The power of the current to overcome chemical affinities increases with this density. If for instance a constant current is passed through a solution of chlorid of chromium in water, it depends on the section of the reducing pole or electrode whether we obtain hydrogen, sesquioxyd of chromium, protoxyd of chromium, or metallic chromium. The relative mass of the constituents of the electrolyte exerts also a not less important influence. As the unit of measure for the density of the current, Bunsen assumes the current having the absolute intensity 1 distributed upon 1 square millimeter. The intensity of the current was measured by a Weber's tangent's-compass, and reduced to absolute measure by the formula $I = \frac{R\,T}{2\pi}$ tan. φ, in which R is the radius of the ring in millimeters, φ the deviation of the needle, and T the horizontal component of the earth's magnetism expressed in Gaussian units. The value of T was determined by the decomposition of water with the help of the electro-chemical equivalent of water. If A represents the quantity of water decomposed in t seconds by the current whose intensity is I, a being the electro-chemical equivalent of water, we have by Faraday's law, $I = \frac{A}{a\,t}$, and this equation combined with the former gives $T = \frac{2\pi\,A}{a\,t\,R \text{ tan. } \varphi}$. For the density of the current whose absolute intensity is I, the electrode having a section O measured in square millimeters, we have $D = \frac{I}{O} = \frac{R\,T}{2\pi O}$ tan. φ. Bunsen determines the quantity of water decomposed by the loss of weight of the flask in which the decomposition takes place; he finds that the formation of peroxyds of hydrogen may be easily and completely avoided by adding only a small quantity of sulphuric acid to the electrolyte, and keeping the decomposing cell during the experiment at a temperature above 60° C. The error which arises from the catalytic recombination of the two gases upon the platinum plates may also be completely avoided by first amalgamating the plates and then igniting them till the mercury is completely expelled. In overcoming powerful affinities the author uses a decomposing cell, one pole of which consists of the inner surface of a carbon crucible filled with chlorhydric acid, placed within a porcelain crucible and kept hot in a water-bath. A small earthenware cell within the carbon crucible serves to contain the fluid to be decomposed. A narrow strip of platinum dips into this fluid and serves as the second battery pole, the current being compressed upon this to a great density. With this apparatus and a solution of the mixed proto- and sesqui-chlorids of chromium, metallic chromium is easily reduced in leaves having a surface of 50 square millimeters. These are very brittle, and where they adhere to the platinum have a bright metallic lustre: in ex-

ternal appearance chromium resembles iron, but resists better the action of moist air, and on heating in the air burns to sesquioxyd of chromium. Chlorhydric and sulphuric acid dissolve it with difficulty, forming protochlorid and sulphate of protoxyd of chromium. Nitric acid, even when boiling, scarcely attacks it: its density corresponds almost precisely to that deduced from the atomic volume of the metals of the magnesia group. In one experiment the reduction of the metal took place when the density of the current was 0·067: when this density is diminished a point is soon reached when no metal is reduced, but when there is an abundant production of a combination of the two oxyds of chromium as an almost black uncrystalline powder. Chlorid of manganese is decomposed like chlorid of chromium; the metallic manganese is obtained in large brittle leaves which oxydize in the air almost as easily as potassium. When an amalgamated platinum wire is employed as the reducing pole of the battery in concentrated boiling solutions of the chlorids of barium, calcium, &c. the density of the current may be increased to 1, and even beyond that. In the solution of chlorid of calcium acidulated with muriatic acid, the wire becomes covered with a gray layer of calcium which contains but little mercury. The reduction of calcium is however difficult; that of barium is much easier, and masses of amalgam weighing 1 gramme are easily obtained. This amalgam is solid, silver white, and very crystalline: heated in a current of hydrogen it leaves a dark porous mass, in the cavities of which silver white metallic surfaces are visible. The fused chlorids of barium, strontium and calcium are not reduced by the galvanic circuit like the chlorid of magnesium. The chlorid of calcium, even when fused, obstinately retains water, and thus the negative pole becomes covered with a non-conducting layer of lime. When melted tin is made to form the negative pole, an alloy containing from 8 to 12 per cent. of calcium is easily obtained. The author proposes to continue these experiments with other compounds than the chlorids.—*Pogg. Ann.*, xci, 619, *April*, 1854.

[*Note.*—The illustrious German chemist does not appear to have been acquainted with the experiments made in this country by Dr. Hare on the reduction of Barium, Strontium and Calcium by the galvanic battery, as these are not alluded to in his memoir. The discovery of the influence of the *density* of the current upon the intensity and character of the electrolytic effects is undoubtedly one of very great importance. It appears reasonable to suppose that this influence is also very powerful in the battery cells themselves, as well as in the decomposing cells. In those batteries in which, like Grove's and Daniell's, two metals and two liquids are employed, and in which a reduction takes place at the surface of the negative metal, it would seem that such dimensions should be given to the surface of the negative metal that the electric density shall be exactly sufficient to produce the particular chemical effect required at that surface, whether reduction of copper or oxydation of hydrogen. The writer would furthermore suggest that the explanation of the remarkable effects of polarization produced by the magneto-electric machine and mentioned in the previous abstract, is to be found in the peculiar *density* of the magneto-electric current, under the circumstances mentioned, and not merely in its discontinuous

character. *It is not improbable that the density of the current required to produce a particular chemical decomposition will serve as an accurate and available measure of the force of chemical affinity under various circumstances of temperature, mass and pressure.*—W. G.]

3. *On the losses of weight which minerals undergo by heat.*—H. St. Claire Deville and Fouqué have communicated to the Academy of Sciences a memoir on this subject containing results which if confirmed will prove of much importance for mineral chemistry. The losses of weight which minerals experience by heat may arise from the presence of water of fluorine and of boron. The authors assert that the temperature at which the water is expelled from a mineral lies far below that at which the fluorine begins to volatilize. They employ therefore two lamps, one fed with a mixture of alcohol and oil of turpentine, the other a blast lamp in which the vapors of oil of turpentine are consumed. The former perfectly expels the water without a trace of fluorine; the latter completely drives off the fluorine. The nature of the loss of weight which a mineral containing fluorine undergoes depends upon its constitution. The authors prepared a basic silicate of soda, and fused it over the large lamp, with a weighed portion of pure fluorid of calcium. In this case the whole of the fluorine was volatilized as fluorid of sodium, while a silicate of lime remained, containing the whole of the silica. A second experiment was made with topaz: this lost over the large lamp 23 per cent. of pure fluorid of silicon. The authors ingeniously showed this by placing the topaz in the centre of a combination of inverted concentric platinum crucibles, the intervening spaces being filled with lime. This system lost no weight on ignition and the lime was found converted into a mixture of silicate of lime and fluorid of calcium, the fluorine and silicon being in the ratio of 3 to 1 in equivalents. A silicate of alumina remained from the topaz which withstood the heat of melting platinum. The authors assume $4Al_2O_3$, $3Si(O, Fl)_3$ as the true formula of topaz, and explain the observed differences between the varieties of this mineral by considering fluorine as replacing oxygen in greater or less quantity. Between those minerals which lose fluorid of silicon and those which retain all their silicon, there is another class composed especially of minerals which contain lithium. Lepidolite gives over the large lamp a very intense red flame which proves that lithia is volatilized, and explains the variations in those analyses in which lithium and fluorine have both been determined after ignition.—*Compt. Rend.*, xxxviii, 317. W. G.

4. *Illustrations of Chemical Homology.*—Under this title Mr. T. Sterry Hunt presented a paper to the American Association at Washington in May last. It was an extension of some of the views already advanced by him in this Journal for March and September, 1853, and discussed many points with regard to the homologies of organic and mineral species. He considers the neutral and basic salts of an acid and base, as members of a homologous series. Thus the three nitrates of lead in the ordinary notation are, PbO, NO_5; $HO, 2PbO, NO_5$; and $HO, 4PbO, NO_5$. Doubling these we may write them, $(Pb_2O_2)N_2O_{10}$; $H_2O_2, (Pb_2O_2)_2 . N_2O_{10}$; and $H_2O_2, (Pb_2O_2)4 . N_2O_{10}$; so that, excluding the water, the common formula of the lead nitrates becomes

$(Pb_2O_2)n \cdot N_2O_{10}$. These salts vary in solubility and in physical characters, but resemble each other in yielding nitric acid and oxyd of lead, as results of their decomposition, and are completely analogous to the homologous series of Gerhardt, which differ by $n(C_2H_2)$. From the relations between basic and hydrated salts, the same view is to be extended to the latter, and species differing by $n(O_2H_2)$, and by $n(O_2M_2)$, may thus be homologous. The above formulas are intended to involve no hypothesis as to the arrangement of the elements, for in the author's view, each species is an individual, in which the different species that may be obtained by its decomposition, have no actual existence.

He regards those silicates which like eudialyte, sodalite, and pyrosmalite, contain metallic chlorids, as oxychlorids, $=(M_2O_2)n \cdot M\,Cl$, and nosean, haüyne and lapis-lazuli as basic sulphates $=(M_2O_2)n\,S_2O_8$, while cancrinite, and perhaps some scapolites, are basic carbonates. All other silicates are reducible to the same type as the spinels, $n(M_2O_2)$, the formula of silica being SiO_2 with an equivalent weight of $7{\cdot}1+8{\cdot}0=15{\cdot}1$. Boracic, titanic, niobic and tantalic acids are reduced to the same formula as silica.

Homœomorphous species have similar equivalent volumes, so that the density in species thus related, enables us to determine their comparative equivalent weights, and to fix their positions in a homologous series. The proportion between the silica and the other oxyds may vary greatly in related species, while the characters of the genus or the order are preserved. This is illustrated in hornblende, diopside, and aluminous pyroxenes like hudsonite, where the oxygen of the bases being represented by 8, that of the silica is respectively equal to 18, 16 and 9.

The triclinic feldspars, of which albite and anorthite may be taken as the representatives, furnish another example; the one is a lime feldspar, $\dot{C}a^3\,\ddot{S}i + 3\ddot{A}l\,\ddot{S}i$, and the other a soda feldspar, $\dot{N}a\,\ddot{S}i + \ddot{A}l\,\ddot{S}i^3$; multiplying the first by $\frac{4}{3}$, and the second by 4, and expanding, they are reduced to a common formula $M_{64}O_{64}$. Petalite, a lithia feldspar, also enters into the same formula, with a similar equivalent volume, while orthoclase belongs to a homologous genus, which is $M_{60}O_{60}$. The formulas with their equivalent weights, densities, and volumes, are as follows:

		Eq. wt.	Dens.	Eq. vol.
Anorthite . .	$(Si_{32}al_{24}Ca_8)O_{64}$	$= 1118{\cdot}4$	$\div 2{\cdot}76$	$= 405{\cdot}0$
Albite . . .	$(Si_{48}al_{12}Na_4)O_{64}$	$= 1054{\cdot}4$	$\div 2{\cdot}62$	$= 402{\cdot}4$
Petalite . . .	$(Si_{51}al_{10}Li_3)O_{64}$	$= 983{\cdot}9$	$\div 2{\cdot}45$	$= 401{\cdot}5$
Orthoclase . .	$(Si_{45}al_{12}K_3)O_{60}$	$= 1026{\cdot}7$	$\div 2{\cdot}56$	$= 402{\cdot}6$

The above formula of petalite requires silica 78·27, alumina 17·31, lithia $4{\cdot}42 = 100{\cdot}00$, and that of orthoclase, silica 66·18, alumina 20·02, potash $13{\cdot}80 = 100{\cdot}00$, which agrees with a large number of analyses, although there are varieties which appear to contain more alkali.

Between anorthite and albite, may be placed vosgite, labradorite, andesine, and oligoclase, whose composition and densities are such that they all enter into the same general formula with them, and have the same equivalent volume. The results of their analysis are by no means constant, and it is probable that many, if not all of them, may

be but variable mixtures of albite and anorthite. Such crystalline mixtures are very common; thus in the alums, aluminium, iron and chromium, potassium and ammonium, may replace one another in indefinite proportions, and the hydrated sulphates of copper and magnesian metals, are obtained in similar mixtures. Heintz has shown by fractional precipitation, that there are mixtures of homologous fatty acids, which cannot be separated by crystallization, and have hitherto been regarded as distinct acids. The author insists that the possibility of such mixtures of related species, should be constantly kept in view in the study of mineral chemistry. The small portions of lime and potash in many albites, and of soda in anorthite, petalite, and orthoclase, are to be ascribed to mixtures of other feldspar species.

The paper concludes with a view of the homologous and homœomorphous relations of the metallic oxyds, titanates, niobates and tantalates, and their affiliations with the silicates.

II. Mineralogy and Geology.

1. *Notice of von Kobell's paper on Series of Isomorphous and Homœomorphous Forms*, published in Schweigger's Jour., vol. lxiv, p. 410; by J. D. Dana.—In this early paper of von Kobell, published in 1832, (but which we had not the privilege of consulting till receiving quite recently a translation from Mr. G. J. Brush, now in Munich,) the subject of the relations of form among minerals of the Dimetric and also of the Hexagonal System is presented in a similar manner as regards the general principle, to that of the writer in the last volume of this Journal. Von Kobell aims to show that in the dimetric and hexagonal systems as in the monometric, there are many species related in form that are unrelated in composition. After some details with reference to certain analogous compounds, as Scheelite and Wulfenite, he enters upon a comparison of dimetric species differing widely in composition, and shows the similarity of form, of Meionite and Wernerite; Copper Pyrites and Braunite; Apophyllite and Anatase: to the group of Copper Pyrites, and Braunite, he afterwards adds Nagyagite, Corneous Lead, Idocrase, Uranite, Mellite; and to Apophyllite and Anatase the species Zircon, Rutile, native Calomel. Thus relations among the dimetric species are brought out, very similar to those presented by the writer in his recent paper.

Von Kobell next takes up the Hexagonal System, and shows the relations between the Corundum group (including, as first shown by Mitscherlich, Specular Iron and Ilmenite) and the Calcite group. The relation of the Calcite and Corundum groups is shown by comparing $\frac{4}{5}$R of Calcite with $-\frac{1}{2}$R of Specular Iron; and the rhombohedron of Copper Mica 68° 41′ is shown to be near -2R of Specular Iron; while again Red Silver Ore is observed to be near Calcite in angle, the arsenical variety giving the angle 107° 40′ to 107° 36′. Von Kobell also further remarks that "other hexagonal species may be brought into similar series, if differences of $1\frac{1}{2}$ degrees be neglected. The series of quartz and corundum are united by beryl with the series of apatite and pyromorphite; magnetic pyrites and chlorite both have pyramids with the basal angle 120°." The fact of a relation in form among

hexagonal species is here exhibited, as in the writer's recent paper already referred to; but the relation is somewhat different in kind, and depends on less simple ratios, as is seen in the method of comparing Calcite and Corundum. The relations between hexagonal and dimetric species is not touched upon.

Von Kobell thence concludes that entirely different compounds may have closely similar or identical forms; and similarity of form is, therefore, not an indication of similar chemical composition. This important conclusion is also supported by a brief mention of some examples of similar import, in the trimetric and monoclinic systems, as that of Borax and Augite; Prehnite, Antimony Glance and Epsomite; Amphibole and Augite.

2. *On the Chloritoid of Bregratten in the Tyrol, and Clinochlore of Markt Leugast in Bayreut;* by Prof. von Kobell, (J. f. pr. Chem., 1854, lxii, 92, from the Gel. Anz. k. k. Bayr. Akad. d. Wissensch.)—The Bregratten *Chloritoid* has a blackish-green color and is associated with quartz. An analysis, made with special precautions, afforded Prof. v. Kobell:

	$\ddot{S}i$	$\dot{A}l$	$\dot{F}e$	$\dot{F}e$	$\dot{M}g$	$\dot{H}$	
	26·19	38·30	6·00	21·11	3·30	5·50	= 100·40
Oxygen	13·59	17·90	1·79	4·68	1·32	4·88	

from which results he deduces the formula

$$(\dot{F}e, \dot{M}g)^3\ \dot{A}l + 2\dot{A}l\ \ddot{S}i + 3\dot{H} \quad \text{or} \quad \dot{F}e\,\dot{A}l + 2\dot{A}l\,\ddot{S}i + \dot{M}g^3\,\dot{H}^3.$$

The *Clinochlore* shows lines of cleavage parallel to a rhombic prism of nearly 120°, and also parallel to the shorter diagonal, and like the clinochlore of Pennsylvania, it is optically biaxial. B.B. fuses on the edges to a pale yellowish mass. In borax dissolves with intumescence, and slowly yields a chrome-green glass. Fuses imperfectly with soda to a yellowish mass. Decomposed perfectly by sulphuric acid, and imperfectly by muriatic. The analysis afforded von Kobell:

	$\ddot{S}i$	$\dot{A}l$	$\dot{F}e$	$\ddot{C}r$	$\dot{M}g$	$\dot{F}e$	$\dot{H}$	
	33·49	15·37	2·30	0·55	32·94	4·25	11·50	= 100·40
Oxygen	17·38	7·18	0·69	0·17	13·17	0·94	10·22	

giving him the formula

$$3\dot{M}g^3\ \ddot{S}i + 2\dot{A}l\ \ddot{S}i + 3\dot{M}g\ \dot{H}^3.$$

This Clinochlore occurs in Serpentine.

3. *On the Alteration of Scapolite;* by G. v. Rath, (Pogg., xc, 288.)—This paper is a continuation of an extended examination of Scapolites by von Rath. The author here gives analyses of different altered Scapolites.

(1.) *Mica* after Scapolite, from Arendal, Norway. The altered crystals are 6 inches in length; they are covered with mica crystals, and consist of an aggregation of scales of the same. G. =2·833. H. =2·5.

Analysis—

$\ddot{S}i$	$\dot{A}l$	$\dot{F}e$	$\dot{C}a$	$\dot{M}g$	$\dot{K}$	$\dot{N}a$	$\dot{H}$	$\dot{C}a\,\ddot{C}$
44·49,	24·91	4·84	2·14	0·36	6·71	1·11	3·44	11·11=99·11

whence the oxygen ratio for $\dot{R}, \dot{R}, \ddot{S}i$, 0·38 : 2·12 : 4. The analysis shows an addition of 6·71 of potash, not found in the unaltered Scapolite of Arendal, besides a removal of the larger part of the lime by carbonic acid, and also other changes.

(2.) *Yellow Scapolite* from Bolton, Mass. This is a massive, pale yellowish variety. G.=2·787. Analysis gave

$\dot{S}i$	$\dot{A}l$	$\dot{F}e$	$\dot{C}a$	$\dot{M}g$	$\dot{K}$	$\dot{N}a$	$\dot{H}$	$\dot{C}a\,\ddot{C}$
49·99	23·01	1·64	3·35	1·73	7·09	0·35	4·23	7·80 = 99·19

Here also the addition of potash is large, being 7·09 per cent., and the composition is near that of Potash mica.

A *Red Scapolite* of Arendal, is next described. The analysis affords 4·42 of potash and 4·31 of soda. A *black scapolite* from Arendal afforded no alkalies and a large amount of magnesia, showing a removal of soda and an addition of the magnesia. Another from the *same* locality, Arendal, has the constitution of epidote.

[The yellow scapolite of Bolton is very similar in composition to the Algerite of Hunt, and sustains the view of Whitney that Algerite is a result of the alteration of Scapolite. It is a singular fact that Bolton alone has afforded to analysts the oxygen ratios for $\dot{R}$, $\ddot{R}$, $\ddot{S}i$, as follows:

1 : 2 : 4 1·2 : 2 : 4 1·2 : 2½ : 4 1 : 2½ : 4 1 : 3 : 6 1 : 2½ : 6 1 : 3·9 : 9

a wide variation of composition for a single locality, but readily explicable when the ease with which scapolite undergoes alteration from atmospheric agencies or infiltrating waters is considered.—J. D. D.]

4. *Report on the Salt and Gypsum of the Preston Salt Valley of the Holston River, Virginia;* by Prof. H. D. Rogers. Boston, 1854.—This is an interesting topographical, geological and economical sketch of the saliferous deposits of southeastern Virginia, embraced in a rich valley of the Apallachians in Washington county. The deposits of salt, salt water, and gypsum occur on the line of an extensive and profound dislocation of the strata parallel to the main mountain ridges and to the trend of the plain which they enclose. The soil of the valley is wet and peaty; and beneath it to an enormous depth, there appears to be no solid rock, but a deposit of clay and earth, imbedding in places, large bodies of rock salt and of gypsum and saturated in its lower portions with highly concentrated brine. The vertical movement or heave of the rocks along the line of the fracture before mentioned is excessively great, inasmuch as the strata on the southeastern side of the fissure belong to the great Auroral magnesian limestone, the lowest of the Appalachian limestones, equivalent to the Cambrian or lowest fossiliferous system of England, while those on the other or northwestern in immediate contact with them are the saliferous and gypsiferous beds of our Appallachian umbral series, the near representatives in age of the European carboniferous limestone, and in original position of horizontal stratification removed by many thousand feet of interposed deposits from the older lower masses, into contact with which they have been forced by the heave along the fracture. The author in connection with Prof. Wm. B. Rogers, has made approximate measurements of the Preston salt valley, from which he infers that this vertical displacement by which the older Auroral strata have been inverted from the southeast upon the umbral beds, amounts to not less than 8000 feet.

Five productive wells are now in use in this valley, bored to the moderate depth of 200 to 300 feet, and four inches diameter. In the case of one well, over 300 feet of rock salt, divided by a little clay, were passed through without tapping any brine at all. The brine rises in the

well to within 45 feet of the surface, from which depth it is lifted by pumps and distributed to the evaporating houses by wooden logs. The quantity of pure salt now produced is 300,000 bushels per annum, worth about 50 cents. This is the present demand; vastly larger quantities can be supplied when there is a market. It appears from the statements of Prof. Rogers that the brine of these salt wells is stronger and purer than any other brine known in the United States. The usual proportion of salt in it is about 23 per cent.; eighteen gallons yielding one bushel of salt. It is entirely free from the chlorids of calcium and magnesium, and as a consequence of this, and of the absence of iron, the salt is dry and colorless—no appreciable quantity of *bittern* being produced in the evaporation. Gypsum is the only foreign ingredient present in any notable quantity, and this is easily and completely separated in the process of evaporation.

Prof. Rogers estimates the quantity of merchantable gypsum fit for agricultural and other uses, and within 60 feet of the surface, as determined by numerous borings, to be not less than 500,000 tons. It occurs, as this mineral usually does, in detached masses, often of considerable extent, but forming no part of the regular bedding of the adjacent rocks. Three beds of *anhydrite* have been developed on the Preston estate, confirming the view that volcanic steam and thermal waters have been concerned in the production of the gypsum beds. In view of all the facts relative to the actual mode of occurrence of the deposits of gypsum and anhydrite, along the line of this great fissure, and of the known analogies of existing hot springs, Prof. Rogers says: "may we not even venture to imagine that the several gypsum-containing plains or valleys along the line of the great fracture, the Preston salt and plaster valley in particular, were so many basins, filled at times with turbid water kept heated and replenished by constant or by intermitting boiling springs and jets of volcanic steam charged with salt and sulphate of lime extracted from the rocks." Prof. Rogers sustains this probable suggestion by several sufficient chemical reasons, and ingenious arguments.

A report on the Geology and mining resources of that part of the Lackawanna Coal Basin which includes the lands of the Delaware, Lackawanna and Western Railroad Company, and those of the Lackawanna Iron and Coal Company in Pennsylvania, by the same able author, we are compelled to pass without further notice, although an abstract of its contents would not fail to interest and instruct our readers. Twelve overlaying beds of workable coals are described, with an aggregate least thickness of 54 feet, yielding 37½ feet of good coal, with a yield of 42,000 tons to the acre. Upon this solid foundation of prosperity, accompanied with abundant good iron ores and other minerals, the manfacturing town of Scranton has been built, and on a scale of magnitude unexampled hitherto in the United States.

5. *The Metallic Wealth of the United States described and compared with that of other Countries;* by J. D. WHITNEY. 8vo, pp. 510. Lippincott, Grambo & Co. 1854.—We have read this volume with pleasure and no small share of instruction. While, as its title implies, the main object of the author is to unfold the metallic resources of the United States, he has given us by way of comparison a connected and

as far as space and facts would permit, a complete view of the same resources in all countries. The plan of the work is simple and methodical. Each metal, beginning with gold and ending with iron, is taken up, and after a discussion of the mineralogical and geological occurrence of its ores, their distribution throughout the world is described, with more or less fullness, according to the importance of the subject. Carefully prepared statistical tables follow, showing the annual production both in quantities and values for all countries where it has been possible to procure accurate information. The work bears marks of diligent research both in the archives of mining and in the mineral districts themselves. A vast mass of valuable facts has been methodised, conflicting statements have been explained or set aside and the maze of local terms for values, quantities and qualities has been translated into intelligible English.

An introductory Chapter of 77 pages is devoted to a concise and clear explanation of the laws which characterize the deposits of the metals and their ores, and a brief description of the general methods followed in mining operations. This is a most useful chapter, and will be read with instruction by all who feel any interest in this great branch of our national wealth. In it the technical laws of the miner's art are explained, and clear ideas are given as to the various modes of occurrence of the useful ores and the importance in all mining operations of carefully recognizing these distinctions in their practical bearing.

Each mine in the United States is taken up under its appropriate head and described as completely as the materials which could be collected by personal examinations and from official or reliable published accounts would allow. In doing this the author does not withhold his opinions wherever he has deemed it advisable to express them. He alludes to the value of some of our metalliferous deposits with a degree of candor and sincerity which must command respect even where it fails to convince those of an opposite opinion. His remarks in the Introduction, upon the false and fraudulent schemes by which the public are deceived or swindled and the whole subject of mining brought into disrepute, will meet with universal approbation.

The Chapter on the Copper mines of Lake Superior contains a very complete summary of the progress and present position of the most remarkable mining development ever made in the eastern United States. The remarks on the present and future value of this region derive great additional interest from the intimate personal knowledge of the author respecting this remarkable region. The same may be said of the Chapter on the Lead districts of Wisconsin and Missouri, which are regarded as having reached their point of highest development and not as possessing in any respect the permanent character belonging to true veins. The data respecting the Iron manufacture of the world are particularly full and valuable. It is not possible in a short notice to review the contents of Mr. Whitney's able volume with such detail as to make them fully known. It is a book to be studied and will be on the table of every person who, from whatever motive, may desire to know our resources in metallic wealth.

A general summary at the close of the volume is given, accompanied by a tabular statement of the estimated amount and value of metals

produced throughout the world in 1854. The metals selected are gold, silver, mercury, tin, copper, zinc, lead and iron. The aggregate of these are as follows:

Gold.	Silver.	Mercury.	Tin.	Copper.	Zinc.	Lead.	Iron.
lbs. troy.	lbs. troy.	lbs. av.	tons.	tons.	tons.	tons.	tons.
481,950	2,965,200	4,200,000	13,660	56,900	60,550	133,000	5,817,000

The product of the United States in gold is set down at 200,000 pounds, Australia and Oceanica at 150,000, and Russia at 60,000, Mexico and South America 47,100. Of silver, the New World supplies 2,473,700, pounds, leaving only the small residue of 491,500 lbs. for all other countries. Of mercury, Spain gives the world 2,500,000 lbs. and the United States 100,000 lbs. England and Australia furnish over half of all the copper produced by the world: the present product of the United States being in this metal only 3,500 tons. Prussia and Belgium furnish four-fifths of all the zinc used in the world (viz. 16,000+33,600 tons.) Lead is distributed between Great Britain, Spain and the United States in the ratio of 4, 2, 1 (viz. 61,000, 30,000 and 15,000 tons each.) England furnishes more than half the Iron of the world, 3,000,000 tons, and the United States 1,000,000 tons. France is the next most productive country in iron, 600,000 tons. Russia produces but 200,000 tons, and Sweden 150,000 tons, quantities bearing a very small relation to the celebrity of product of those countries.

The following table exhibits the comparative value of the metallic productions of different countries, from which may be seen the ratio of their production, as compared, first, with that of this country taken as the unit, and, secondly, with that of Great Britain.

	Value of Metals produced.	Ratio of production to that of	
		U. States.	Gr't. Britain.
United States,	$79,827,000	1·	5·6
Great Britain,	96,169,800	1·205	·1
Australia,	39,428,000	·494	5·12
Mexico,	30,480,000	·382	1·3
Russian Empire,	25,240,000	·316	1·6
France,	15,252,500	·191	4·15
Chili,	13,144,000	·165	2·15
Rest of South America,	16,176,000	·203	1·6
Austrian Empire,	11,708,000	·147	1·8
Prussia,	9,680,000	·121	1·10
Belgium,	9,375,000	·118	1·10
Spain,	8,016,416	·100	1·12
Sweden and Norway,	5,460,896	·068	1·17
Saxony,	1,455,000	·018	1·67
Hartz,	1,147,588	·014	1·86
Italy,	832,500	·010	1·120
Switzerland,	375,000	·005	1·240

The great importance of our own metallic resources will be at once apparent from an inspection of the above table. It will be seen that we are second only to Great Britain in our production, as we are also in our consumption, of the metals. The two great Anglo-Saxon countries stand far before all others; and Australia, a colony of England of but a few years growth, is the next competitor on the list. As our production of gold which now forms so important an item of our metallic wealth, falls off, as it assuredly will, the deficiency may be more

than made up by the development of our resources for the production of other metals.

Mr. Whitney's work is certainly a most important addition to scientific literature, not merely in the English language but in all countries. It is a model of pure scientific style, and the mechanical part of the work is equalled only by the faultlessness of the proof-reading. In a pretty careful perusal of the whole volume we have failed to detect the first typographical error.

6. *City of San Salvador destroyed by an Earthquake.*—On the night of the 16th of April last, the city of San Salvador was completely destroyed by an earthquake. The population of the city in 1852 was estimated at 25,000. The following facts relating to the country and the catastrophe are cited from different sources.

The hills around the plain of San Salvador are covered with verdure, which, as the dews are considerable, keeps green through the dry as well as the rainy season. The city, with its white houses and churches, seemed, therefore, to be set in living emerald. About three miles to the westward of the city is the great volcano of San Salvador. The cone, which rises on the northern border or edge of the crater, is (approximately) 8,000 feet in height. The volcano proper, however, is a vast mass, with a broad base of irregular outline, its summit serrated by the jagged edges of the crater, and is much less in altitude than the cone. This cone seems to have been formed by ashes and scoriæ thrown out of the crater, which is represented as a league and a half in circumference, and a thousand *varas*, or three thousand feet deep—almost large enough to receive the whole mass of the volcano of Vesuvius. At the bottom of this crater is a considerable lake of water. Very few persons have had the temerity to venture into the chasm of the volcano, and none of these are likely, judging from the accounts which they give of their efforts, to repeat the undertaking. Two Frenchmen, who ventured down a year or two since, became exhausted and incapable of returning. They were rescued, with great difficulty, by a detachment of soldiers from the garrison.

The entire line of the coast range of mountains or hills, in San Salvador, bristles with volcanos. Thirty-five miles to the eastward of the city is the great forked peak of San Vicente, 10,000 feet in height; and thirty miles to the westward, on the same line, is the vast bulk of the volcano of Sta Ana with its dependent peak, the volcano of Isalco, which is in a state of constant eruption, and is called "El Faro de San Salvador," the Lighthouse of San Salvador. Besides these are numerous other volcanos, occurring, in conjunction with those just named, in the following order, commencing at the eastern extremity of the State: —San Miguel (active), Chinemeca, Sacatecoluca, Tecapa, Usulutan, San Vicente, San Salvador, Guasapa, Izalco (active), Sta Ana, and Apenaca, besides some others of less note, to say nothing of extinct craters, volcanic orifices, or extinct vents, which are now generally filled with water, constituting lakes without outlets, and of which the water is brackish. One of these, called "Joya," occurs about four miles to the southwest of the city of San Salvador.

It would be impossible to describe here the numerous active vents, emitting smoke, steam and sulphurous vapors, which occur at or near

the bases of some of these volcanos, and which are called "*Infernillos*," literally "little hells." There are, also, numerous other volcanic phenomena and results, of exceeding scientific as well as popular interest, but which it would exceed the scope of this article to describe adequately. In a word, it may be said with truth, that San Salvador comprehends more volcanos, and has within its limits more marked results of volcanic action, than probably any other equal extent of the earth. For days the traveller within its borders journeys over unbroken beds of lava, scoriæ and volcanic sand, constituting, contrary to what most people would suppose, a soil of unbounded fertility, and densely covered with vegetation.

San Salvador stands, or rather stood—for its destruction has been so complete as to justify the use of the past tense—upon a table land wholly made up of scoriæ, volcanic ashes, sand and fragments of pumice, overlying, to the depth of hundreds of feet, the beds of lava which had flowed from the volcano before their ejection. Those who have seen the scoriaceous beds, which buried Pompeii, can form an accurate idea of the soil on which San Salvador was built.

The channels of the streams are worn down to a great depth through this light and yielding material, and constitute immense ravines, which render the approaches to the town almost impassable, except at the places where gradual passages are cut down on either side, paved with stone, and sometimes walled, to keep them from washing out and becoming useless. Some of these approaches are so narrow that it is customary, when mounted, to shout loudly on entering, so as to avoid encountering horsemen in the passages, which are frequently so narrow as to preclude either passing or turning back. San Salvador has more than once owed its safety, in time of war, to these natural fortifications, which confounded the enemy with their intricacies and difficulties, while affording means of defense to the inhabitants.

The facility with which the soil above described washes away has been the cause of considerable disasters to San Salvador. During a heavy rain of several days duration, called a "*Temporal*," which occurred in 1852, not only were all the bridges which crossed a small stream, flowing through one edge of the town, undermined and ruined, but many houses destroyed in the same manner. One of the principal streets, extending into the suburbs, began to wash at its lower extremity, and the excavation went on so rapidly that no effort could arrest it. A considerable part of the street became converted into a huge ravine, into which the houses and gardens on either side were precipitated. The extension of the damage was guarded against, when the rains ceased, by the construction of heavy walls of masonry, like the faces of a fortification. How serious an undertaking this was regarded, may be inferred from the fact, that its completion was deemed of sufficient importance to be announced in the annual message of the President.—*Correspondence of the N. Y. Herald.*

The attention of the dwellers and sojourners upon the south-western part of that elevated plain which lies above the city of San Salvador, upon the 12th and 13th of April last, was forcibly called to a hollow, rolling, subterranean sound, which was repeated at intervals, and at

times continued several minutes without ceasing. It seemed to proceed from the mountain-chain, which extends southwesterly from the neighboring volcano, and forms a semi-circle. The awe-inspiring sound was most distinctly heard at Monserrat and at a little *hacienda* (farm) belonging to a German family, named Bogen, from East Prussia.

About 7½ o'clock on the morning of Good-Friday, (April 14,) two slight shocks of earthquake were felt at San Salvador, and in the immediate vicinity, succeeding each other with little interval, and followed some ten minutes later by one more severe. I saw the roof and walls of my little habitation trembling without at first perceiving the cause. "Es un temblos," said Martino, my young New-Spanish attendant, very quietly. He was a native of the country, and therefore accustomed to a phenomenon which fills the mind of an inhabitant of the North with such profound horror. The environs of San Salvador have a bad name throughout the country, on account of the frequent shaking of the earth, and the natives have given the region a name expressive of the fact. But though these slight shocks are constantly occurring, especially at the beginning and end of the dry season, (December and May,) there has never, since the memory of man, been any instance of these terrible catastrophes, which, as at Lima or Valparaiso, are expected about once in a century to overwhelm the city in total destruction. Beside, the volcano of Isalco, sixteen leagues south of San Salvador, being in constant activity, is considered as a sort of chimney, conducting off the vapors and liquid matter from the vast fires beneath, or, to quote Humboldt, as a safety-valve against destructive earthquakes.

The shocks continued throughout Good-Friday, with pretty regular intervals, about as often as two or three per hour, and having all the same direction, west-southwest to east-northeast. In this direction, at a distance of a short league from the city, and at an elevation of about 500 feet above it, is the great crater of Guscatlan. This crater seems to be of a more ancient formation than that of San Salvador, and is partly filled up by a lake. Here the shocks seem to originate, and not at the volcano.

In San Salvador, where the holy week is celebrated with all possible religious pomp, the people paid little attention to the earthquake that took place on Good-Friday, and were but to a very small extent hindered in their participation in the procession, and in their visits to the cathedral. Still, several times in the course of the day, as the shocks grew of greater force, the devout multitudes were seen flying from the holy halls, pale with terror, rushing in wild haste to the doors; their fear of the subterranean powers overcoming their faith in the celestial.

About 8½ o'clock P. M., the houses were shaken to their foundations, and the roofs began to crack. Walls were filled with fissures, the plastering fell from ceilings, and many tiles were thrown from the roofs. This shock lasted at least some eight seconds, with an undulating motion, and had the houses not been so exceedingly well adapted for resistance to earthquakes, they would probably have come down in masses. These houses are all low, very broad, and of only one story, the walls of loam-mud possessing considerable elasticity, and covered with flexible cane—no better construction being possible to meet the case.

Every body fled into the open air. An hour passed without further motion, yet most of the people resolved to put up their couches in their court-yards in the open air. The shocks continued more or less violent at intervals during the whole night; in the course of the twenty-four hours we counted forty-two distinct ones. On Saturday morning it became quiet again.

The capital of the State of San Salvador is situated at an elevation of 2100 Spanish feet above the Pacific, upon a most fertile plain, about 7 square leagues in extent, on the northwest side of which rises the volcano, hardly a league from the city. Seen from the town, the old fire-mountain forms a most beautiful cone, with a gently sloping summit, crowned to the highest peak with thick forest. The crater is perfectly well preserved, more than half a league in circumference, and partially filled with water. It rises about 1000 feet above the table-land on which it stands. The other hills, both those which belong to the volcanic range south, and those of the semi-circle above mentioned, rise not more than 1500 feet above the level of the plain.

There is no historical account of any period of activity in the volcano of San Salvador. There is a tradition, however, of an eruption of lava having taken place in 1659, which is said to have destroyed and covered with ashes the *pueblo* of Nehapa on the northwestern side. According to other traditions, this was no eruption of fire, but an overflow of mud from the crater.

Easter-Sunday was welcomed by the discharge of rockets and the music of the military bands, while the multitude moved in festive procession to hear high-mass in the cathedral. Most of the houses were beautifully decorated with pisang leaves and branches of palm trees. The "Sanctissimum" was carried in triumph through the streets. A long procession followed, and the senoras and senoritas displayed their most splendid toilets. In the afternoon, the grand procession of saints took place. Colossal statues of saints, carved in wood, and most luxuriously equipped in costly silk dresses, were carried from the churches through the streets; and wherever they chanced to meet, the processions stop to give the saints an opportunity to embrace. The multitudes greet these scenes with extravagant delight, and rockets by the hundred are sent rushing through the air. The good Catholic people devote themselves upon Easter-Sunday, first to religious exercises, then to cheerful enjoyment; and so the day concluded with music, fireworks, and banqueting.

Soon after 9 o'clock in the evening, came a severe shock, more powerful than the severest on Good-Friday, accompanied, during its entire continuance, by a hollow, rumbling noise. Walls tottered to their foundations, bricks and tiles fell to the earth, and many houses were rent with fissures. I was lying in bed, suffering under an attack of ague, and had fallen into a feverish slumber, from which the noise awakened me. At that instant a portion of the ceiling of my room fell, beating me upon my head and face, and for some minutes blinding me with dust. I sprang from my bed, and groped my way in the darkness to the door, which was unfortunately locked. I succeeded at length in finding it, and reached the court-yard in safety, where I found all the other inmates of the house assembled, crying and praying in a breath.

In a few minutes, though, the panic was over again, and one heard even laughter and joking at the sudden consternation and flight from the house. These frightful phenomena occur too often to rouse more than a passing anxiety, even when the shocks are of unusual strength. They seem to be content if their dwellings do not sink at once. Still, the inmates of the houses brought their beds into the open air, and opened the doors of their houses. My next neighbor, a young doctor, remarked, that probably no other severe shock would occur that night; to which a Catholic priest replied, that the house was old, the roof rotten, and caution was at all events commendable. The people of the house went in again, and with open doors returned to their Easter feast, the conversation for the next hour turning almost exclusively upon the horrible "temblos."

In the mean time, I, being sleepless, was looking out upon the nightly sky. The day had, as usual, been very warm; the thermometer rising at noon to 88° Fahrenheit. Heaps of cloud (cumulo-stratus) were piled up mountain-like about the declining moon, but at about 10 o'clock disappeared. The moon was now shining merrily through a clear and calm atmosphere, a few vaporous veils of cloud (cirrus or cirro-cumulus) only, still hung immovable about some points of the horizon. Nothing appeared in the atmosphere to announce any uncommon phenomenon.

Half an hour later, (10½ P. M.) came the frighful shock which laid San Salvador in ruins. It began with a loud noise and undulating motion, the ground moving as if shaken by a subterranean sea. This motion, with its accompanying subterranean thunder, in the same direction with the previous shocks, lasted some ten or twelve seconds. The cracking and falling of roofs made a roar through which the appalling sounds below could scarcely be heard. A colossal cloud of dust arose. The terror, the cries and lamentations of the people were beyond description. Then followed prayers and a universal, loud, wailing invocation to *Maria Sanctissima* and all the saints, and finally a low, lamenting, and supplicating song from thousands of voices rising simultaneously from all the places of refuge to which the multitude had fled for safety.

And now began a scene which my pen is unable to describe. How insignificant appeared now the most frightful points in my past life! how mean appeared all the episodes of war and revolution which I had witnessed in the Old World! There, one had to deal with known agencies, with adversaries of flesh and blood, and not, as now, with unknown powers of the depths of whose existence we hardly are aware.

The shocks continued, sometimes light and sometimes with fearful force, with but short intervals, throughout the night and the next day, at the evening of which their number amounted to 120. I can compare the awful rumbling noise attending them only to heavy discharges of artillery in some subterranean battle. Sometimes the noise was more of a rattling character, and the ground waved for minutes without a real shock. No one thought of goods and chattels; the people trembled still for their lives; the motion of the ground had opened it in all directions, and no one knew but that the next moment a yawning chasm would open beneath his feet and swallow him for ever. After each

succeeding shock the multitude changed their prayers, and called upon some new saint for help. But whether the saints did not hear, whether they could not or would not help, the earth continued to tremble, the subterranean artillery to roar. A few hours more, and the more resolute had become accustomed to the roar, and began to take measures for the public safety, the ravages of the Indians being feared.

About 1 o'clock in the morning, a gentleman of my acquaintance climbed over the ruins of my house into the yard to look after me. Seeing me somewhat better, he proposed a walk in the moonlight through the town. We went first to the market-place. The cathedral was still standing, but the town, I now saw, was involved in one general ruin; not a single house had resisted the last frightful earthquake! The cathedral—a more elegant than imposing building, of the last century—had to a certain extent sustained the shock. But its belfry had been thrown down, its porch was in ruins, its walls were cracked and full of fissures. All the other churches, save that of the old Franciscan convent, had suffered far more severely, and their interiors presented sad pictures of solitude and ruin, being covered with dust and rubbish from the fall of tiles and stones from the heavy roofs. Colossal statues had tumbled from their pedestals, and their splendid and gorgeous robes were dragged in the dirt. There they lay, utterly uncared for by the multitudes who the day before had carried them in triumphant procession. Life and property were at this moment of more importance than images, the worship of which had done so little to arrest the footsteps of the calamity. A wing and a newly finished tower of the university still stood, and strangely enough, the clock was still striking the hours with all due regularity. In the Episcopal palace the ceiling had given way, and the bishop, Don Tomaso Saldana, a man justly admired for his piety and virtues, had fared no better with his consecrated head than we profane. Señor Duenas, ex-President of the Republic, once a monk, then lawyer and diplomatist, and incontestably in capacity the first man in the country, was somewhat more seriously injured.

The streets were now deserted, save by military guards posted here and there, and we found our progress much impeded by the piles of ruin and rubbish. Inside the houses was the quiet of death. The people, fearing to remain even in the widest streets, had collected, high and low, rich and poor, and were seated upon the ground in the centers of the public squares. The stiff Spanish etiquette which generally so completely divides the several ranks of the population, was completely forgotten in this night of terror. Rich men and beggars joined their tears, their cries, their prayers, supplications and hymns, at each new shock of more than common severity.

Don José Maria San Martin, the recently elected President of the Republic, exhibited great presence of mind and resolution, and gave wise and energetic directions for the protection of property. At the corner of the cathedral we met the Franciscan monk, Don Estavan Castillo, a beloved acquaintance of ours, a member of one of the first families in the country. He is the most ingenuous and remarkable man whom I have met in Central America, extremely inquisitive, much given to philosophical speculation, and the possessor of one peculiarly

characteristic trait in common with Pascal; he delighted to talk of the great mysteries of the world, those upon which the thinkers and philosophers of all nations and ages have speculated in vain. Our last conversation on the blind ruling of the powers of Nature seemed singularly adapted to the scene which surrounded us now. The monk pressed my hand in silence. He, with a band of strong fellows, was engaged in a search for persons buried in the ruins, while the bishop and all the clergy of the higher ranks had fled to Cojutepeque. At day-break, already a hundred bodies had been drawn from the rubbish, but what the real number of deaths was I have not yet been able to learn. That several thousands were not killed, we must attribute to the caution inspired by the severe preliminary shock above mentioned; nor in all probability, would the writer of these lines, without that warning, have ever held the pen again, as the last principal shock brought down the heavy ceiling of his dwelling.

On Easter-Monday the rising sun illuminated a most sorrowful scene. The inhabitants, pale with fear and fatigue, were wandering at random through the town without shelter or place of rest. The greater part fled in the direction of Apopa and Cajutepeque, leaving every thing behind. Among the women, who were mostly in extreme négligée, I noticed the wife of the President, entreating her husband to flee also from this place of horror. The President, however, remained faithful to his duty, and continued to act with energy. Most of the State prisoners perished, owing to their inability to leave their rooms; and in one case where two were chained together, one was killed, and the other forced to bear his body until relieved of his horrid burden by some police officers. A court-martial held under a tent in the University-place, sentenced every thief caught in the act of stealing and proved guilty by two witnesses, to be shot.

The ruins of San Salvador no longer affording me shelter, I returned next morning to the *hacienda* of an acquaintance on foot, as all the mules had either been stolen or were fully employed by the merchants in removing goods, and therefore not to be procured for any amount of pay. During my walk thither there were four shocks, one of which was one of the severest of all, lasting about seven seconds, and being accompanied, as usual, by a loud detonating sound, just like the noise accompanying the vollies at Vesuvius, to one standing near the crater at a time when volumes of smoke and stones are hurled into the air. My convictions grow stronger every moment that the seat of this subterranean action is very near, and that the vapors and glowing masses of the depths are seeking some new outlet.

Every fugitive of whom we asked the reason of this hurried flight, going off as they did with no thought of even the most necessary articles of comfort, answered alike, that after the destruction of the town, the bishop had said, that the entire region around San Salvador would, "before another moon had passed, sink into the depths of the earth." This prophecy has not, however, been fulfilled. The new moon is shining upon the ruins of the town, and upon the haciendas scattered over the plain. But the earthquakes and the subterranean noises still continue, and upward of a hundred shocks are felt each day. In all probability the powers below are striving to burst the crust of the earth,

and build up a new chimney for their gases, vapors, and molten masses. Woe to him who lives within the reach of the new crater.

P. S.—A month has passed since the above sketch was written. The shocks have decreased in number and intensity, but still they are occasionally felt, and the sounds below are heard. None but the poorest of the population of San Salvador have ventured to return to their former habitations.—*M. Wagner, in N. Y. Tribune.*

III. Botany.

1. *Hooker's Icones Plantarum*, vol. x, plates 901–1000. (London, Pamplin, 1854.)—This volume closes an important work, which, like most botanical publications not of an elementary character, in England especially, has been carried on at a heavy pecuniary sacrifice on the part of the liberal and distinguished author. This last volume is exclusively devoted to Ferns, can be purchased separately, and makes a *pendant* to Sir William Hooker's Species Filicum, being of the same 8vo size. Three of our own rare Ferns are figured in this volume; viz., *Asplenium pinnatifidum*, Nutt.; *Anemia Mexicana*, Klotzsch (No. 572 of Lindheimer's Texan collection, and No. 826 of Wright's); and the pretty little *Trichomanes Petersii*, described last year in this Journal. A. G.

2. *J. D. Hooker's Flora of New Zealand*, part 6, published in June, completes the account of the true *Mosses*, by Mr. Wilson, and contains the greater part of the *Hepaticæ*, which are elaborated by Mr. Mitten. Thirteen more plates are devoted to Musci and Hepaticæ, and about six species are illustrated on each plate. The remaining plates are devoted to Fungi and Algæ. A. G.

3. *Genera Plantarum Floræ Germanicæ Iconibus et Descriptionibus Illustrata.*—This well-known work, begun in 1833 by the late Th. Nees von Esenbeck, continued after his death by Prof. Spenner, after his death by Putterlick and Endlicher down to fasc. 24, and since their decease suspended for several years, is now "conjunctis studiis plurium auctorum continuatum." We have seen nothing of fasc. 25 and 26, but are told that they were issued a few years ago, edited by Prof. Schnizlein. Fasciculus 27, published in 1853 by R. Caspary of Berlin, has come to hand, and comprises 16 genera of *Cruciferæ*, one of *Ranunculaceæ* (Caltha), and 2 of *Papaveraceæ*. The letter-press is for the most part increased to four pages for each genus, and one or two genera have two plates devoted to their illustration. The plates, 20 in number, are crowded with details, particularly of the ovules, ovary, and embryo, and are apparently extremely faithful, but not very elegant, owing to the style of engraving adopted. Fasc. 28, by Prof. Schnizlein of Erlangen (1854), is devoted to several of the earlier monopetalous orders: the letter-press and the plates accord with those of the later preceding parts, and have none of the elaborate details of development given by Caspary. There is some prospect that this valuable work will now go on steadily. We wish we could say as much for the similar undertaking in our own country. A. G.

4. *Floræ Danicæ Supplementi fasciculus I*, 1853.—This great national work, carried on from 1761 to 1845, and comprising 14 folio

volumes with 2460 plates, is to be extended, at the suggestion of Prof. Fries, so as to embrace Swedish and Norwegian plants not already figured. Professor Liebmann is the editor. The 1st fasciculus of this supplement contains 60 plates, a great part of them containing two plants, very faithfully represented. Almost all the species belong also to the boreal American flora; which gives to the work a peculiar interest in this country. A. G.

5. *The Micrographic Dictionary*, by Drs. GRIFFITH and HENFREY, part II. (June, 1854) has been received from the enlightened and tasteful scientific publisher, Mr. Van Voorst (whose name, by the way, is most deservedly commemorated by a beautiful new genus of the network or lace-like Algæ, recently discovered on the south-shore of Ceylon, by Dr. Harvey). The Introduction (pp. xxv–xl), here brought to a close, treats of *illumination* for the microscope, under various circumstances, and of the methods of *microscopical analysis*, or of determining the structure of microscopic objects, from the appearances which they present under various conditions; of *chemical reactions*, and of *measurement*. The Dictionary, pp. 17–32, extends to the article *analytic crystals*. Of the engraved plates there are two (colored) of *Desmidaceæ*, one of *Diatomaceæ*, one exhibiting the microscopic structure of hairs, and one of *Infusoria*. Evidently the work fulfills its promise, and will be a vade-mecum of the microscopist and general naturalist. A. G.

6. *Linnæa: ein Journal für die Botanik: herausgegeben von* D. F. L. VON SCHLECHTENDAL, which was said to be suspended, now goes on again with renewed spirit. The 26th volume (the 10th of the new series) bears the date of 1853; but the 1st part was not published (as the cover properly informs us) until February, 1854, the second, in April. The two parts received contain a monograph of the small group of *Stackhousiaceæ*, by T. Schuckardt; an elaborate disquisition on *Bouvardia*, and the first page of a critique upon the genera *Panicum* and *Paspalum* in Steudel's new Synopsis Plantarum Glumacearum, by the editor; an attempt at a new arrangement of *Gesneriaceæ*, by Hanstein; continuations of *Plantæ Mullerianæ* (Australian) and *Plantæ Wagenerianæ Columbicæ*, by various authors, and one or two smaller contributions. We hope this long-established and valuable Botanical Journal will now go on with regularity and success.

The *Botanische Zeitung*, a weekly Gazette, edited by von Schlechtendal and von Mohl, now in its 12th year, is well sustained. Its principal articles are upon vegetable anatomy and development, thus leaving the pages of the Linnæa clear for memoirs and longer papers on systematic botany.

A rival German Botanical gazette, now in its second year, is the *Bonplandia; Zeitschrift für die gesammte Botanik*, edited by B. SEEMANN, in London, but published at Hannover, a sheet of 8 or occasionally more pages, in small folio, issued on the first and fifteenth of every month. It is handsomely printed, and is valuable for intelligence. Of late there is some unpleasant controversy between it and the Botanische Zeitung, which is to be regretted. A. G.

7. *Annales des Sciences Naturelles*, etc., *rédigée pour la Zoologie par* M. MILNE EDWARDS, *pour la Botanique par* MM. AD. BRONGNIART

et J. Decaisne.—The twentieth volume closes the third series of this most important of natural-history periodicals; and a fourth series commences with the year 1854. The only fault to be found with the work is, that it is, as usual, behind date, only three monthly numbers having as yet appeared during the current year. But this is much better than in former times, when it had fallen almost a twelvemonth behind. Of the Botanical volumes, the most important articles within the last three years have been: Naudin's monograph of the Melastomaceæ of the Paris Museum, an elaborate revision of the family; Tulasne's papers on the *Antidesmieæ*, on the Ergot of Grasses, the reproductive organs of Fungi, and especially his full and incomparable memoir on the Organography of the *Lichenes* (embracing 240 pages of letter-press and 16 well-filled and admirable plates); Thuret's fine researches upon the antheridia of Cryptogams, especially of the Algæ, &c.; a series of very important papers by Trécul on the origin and development of ligneous fibres in the stems of plants, the mode of increase of dicotyledonous stems in diameter, and on the formation of leaves (in all which the author is winning a high reputation as a vegetable anatomist); some good papers by Garreau on what he calls, perhaps with good reason, the respiration of plants, he confining this term to the decomposition of vegetable products and the conversion of carbon into carbonic acid, which he shows to go on at all times, while the plant is effecting any transformations, although masked in the foliage, under sunshine, by the vast predominance of the opposite process (in which *vegetation* consists); an admirable investigation, by Boussingault and his assistant Lévy, of the composition of the air contained in a fertile or manured soil, showing that the roots of our esculent plants and grain, as ordinarily cultivated, are surrounded by an atmosphere which contains from 20 to more than 240 times more carbonic acid gas than the exterior atmosphere (the direct bearing of which against certain views too hastily propounded by Liebig, in former years, is abundantly evident); some good papers by M. Payen, on the organogeny of the flower in several families; and an article by Lucaze-Duthiers on the nature and development of galls and other abnormal productions of plants caused by the puncture of insects, &c.

The two fasciculi of the 1st volume of the fourth series contain some papers of descriptive botany; one by Groenland of Altona on the germination of certain Hepaticæ, beautifully illustrated; one by Trécul, elaborately investigating the formation of vessels and the so-called radicular fibres under adventitious buds, explaining quite differently from M. Gaudichaud the well-known facts which found their readiest explanation according to the theoretical view of the latter; this is illustrated by three admirable plates. M. Dareste endeavors to prove, with some success, that the red and yellow hues of the Chinese sea are due to the presence of a microscopic Alga, probably identical with the *Trichodesmium erythræum* of Montagne, from which the Red sea derives its occasional hue, and, it is thought, its name. A. G.

8. *Mammoth Trees of California.*—Some details may be expected, from the writer in the Sonora Herald cited in the last number of this Journal, respecting several large Coniferous trees in California. Meanwhile, it may be well to state, first: that the hollowed section of a

trunk exhibited last winter in Philadelphia, and which furnished the principal data of the estimate published in the May number (p. 440), was not taken, as we were led to suppose it was, from the famous giant "Wellingtonia" felled near the head of the Stanislaus river. We learn that it came from a less gigantic tree, which grew much nearer to San Francisco, and that the tree was a true *Redwood* or *Sequoia sempervirens*.

(2.) This tree, although considerably smaller, is apparently as old as, and probably older than the great Wellingtonia, although doubtless not surpassing the estimate already given. For, whereas its oldest wood exhibits about 48 layers to an inch, that of the Wellingtonia, of which we now have specimens, has as few as 25 or 20, or even fewer layers to an inch, at or near the circumference. Moreover, Dr. Torrey has recently had an opportunity to count the layers on a complete radius of the trunk of the famous Wellingtonia now exhibited in New York, and he finds that they are only 1120 in number! From the data which Dr. Torrey has furnished we find that, on the radius examined, the

1st	hundred	layers	occupy a	breadth of	17½ inches.
2d	"	"	"	14	"
3d	"	"	"	12½	"
4th	"	"	"	13	"
5th	"	"	"	16½	"
6th	"	"	"	8¾	"
7th	"	"	"	7¾	"
8th	"	"	"	11	"
9th	"	"	"	10	"
10th	"	"	"	11	"
11th	"	"	"	11¼	"
The remainder of 20 layers occupies over				1	inch.

Eleven hundred and twenty layers on the semidiameter of 135 inches, or 11 feet 3 inches. We had ventured to reduce by more than one-third the accredited statement or estimate that this tree was 3000 years old. The facts show that the tree lacks almost three centuries of being half as old as it was said to be! Its enormous size is owing to its continued rapid growth rather than to any very extraordinary age.

(3.) A comparison of the cones of the two brings to view no adequate generic differences between the *Wellingtonia* of Lindley and *Sequoia*. Unless the male flowers furnish characters, the so-called Wellingtonia will hereafter bear the name imposed by Dr. Torrey, namely that of *Sequoia gigantea*. The flowers, however, are still a desideratum.

A. G.

9. *On the Fossil Plants found in Amber;* by Professor GOEPPERT, (Berlin Academy, Bulletin, 1853, pp. 450–476; Edinb. N. Phil. Jour., lvi, 365.)—Since Prof. Goeppert recognised the *Taxodites dubius* of Sternberg, which occurs abundantly in the plant-bed at Schosnitz, Silesia as the *Taxodium distichum*, Rich., now living in the southern parts of the United States and in Mexico, and found also some fossil Plants from Schosnitz to be identical with living species, thus pointing out the identity of some tertiary plants with the living, he has had the opportunity of examining a collection of 570 specimens of Amber, containing plant-remains, belonging to M. Menge of Dantzig, and 30 specimens

beqneathed by M. Berendt. With these the author has been enabled to raise the number of the species of plants in the Amber Flora from 44 to 163, of which only *Libocedrites salicornioides* and *Taxodites Europæus* occur fossil out of the Amber, and 30 are identical with existing species. The constitution of the Amber Flora, as at present known, is shown in the following table.*

	Number of Species.	Number identical with existing Species.
PLANTÆ CELLULARES.		
I. Fungi	16	4, certainly; perhaps all.
II. Algæ	1	1
III. Lichenes	12	6 or 7 (with species on the E. and W. coasts of Arctic America.)
IV. Musci hepatici: Jungermannieæ } 39 specimens	11	11
V. Musci frondosi	19	2 or 3, certainly; perhaps all.
PLANTÆ VASCULARES.		
III. Cryptogamæ (Acotyledones.)		
Filices.		
Pecopteris Humboldtana, *Göpp. & Behr.*		
IV. Monocotyledones.		
Cyperaceæ.		
Carex eximia, *Göpp. and Menge.*		
Gramineæ.		
Fragments.		
Alismaceæ.		
Alisma plantaginoides, *Göpp. & Menge.*		
V. Gymnospermæ.		
Cupressineæ	20	2
Abietineæ	31†	1
Gnetaceæ	1	
VI. Monochlamydeæ.		
Betulaceæ	2	
Cupuliferæ	10	
Salicineæ	3	
VII. Corollifloræ.		
Ericineæ	22	3
Vaccineæ	1	
Primulaceæ	2	
Verbasceæ	2	1
Solaneæ	1	
Scrophularineæ	1	
Lonicereæ	1	
VIII. Choristopetalæ.		
Loranthеæ		
Crassulaceæ	1	1

The whole Flora as yet known includes 24 Families, 64 Genera; comprising 163 species.‡

The following are the general results of Prof. Goeppert's researches.

A considerable number of tertiary species of plants (especially *Plantæ cellulares*) are still living.

* For the lists of genera and species, see the works above referred to.

† Of these, eight (the species determined from the fossil wood) afford Amber.

‡ The number of species may probably be raised to about 180, by additions from about 50 specimens of which the relations are barely determinable.

The flora of the Amber being destitute of tropical and sub-tropical forms, it is to be referred to the Pliocene period.

The remains only of forest-plants have been preserved in the Amber. This flora much resembles the present, especially in the Cellular plants; the *Cupressineæ*, however, are now almost wholly wanting in our latitudes, and the *Abietineæ* and the *Ericineæ* are not abundant. The four species, of *Thuia*, *Andromeda*, and *Sedum*, which are identical with the living, are indeed northern forms; on the other hand, the *Libocedrus Chilensis* is found on the Andes of Southern Chili.

The flora of the northern parts of Europe, Asia, and America, is at present less rich in species of *Cupressineæ* and *Abietineæ* than that of the Amber, although it possesses some of the species found in the latter; nor are the existing northern species of *Coniferæ* so rich in resinous products as were the trees of the Amber Flora with which the *Dammara Australis* of New Zealand can alone in this respect be compared, the branches and twigs of this tree being stiff with white resin-drops.

If we take into consideration the enormous extent which the forests of

Abies alba,	Abies balsamea,	Abies ovata,	Larix Sibirica, and
—— nigra,	—— Sibirica,	Larix Dahurica,	Pinus Cembra,

at present attain in North America and Northern Asia, we are led to infer a similar extension in former times of the Amber-forests throughout the northern regions; to which, indeed, the wide distribution of amber in the late tertiary deposits of North America, Holland, North Germany, Russia, and Siberia to Kamtschatka, bears evidence.

If we judge from the proportion which the fir-forests bear to the trees of our northern flora generally, we shall infer, from the prevalence of the *Coniferæ* in the Amber, the existence of a very rich flora contemporaneous with the latter, and of which but a small part has as yet been presented to our notice. Germany contains 6800 species of *Cryptogamæ*, according to Rabenhorst, and 3454 species of *Phanerogamæ*, according to Koch. The proportions are—

	THE GERMAN FLORA.		THE AMBER FLORA.	
	Classes.	Species.	Classes.	Species.
Cryptogamæ	8	6800	6	60
	Families.	Species.	Families.	Species.
Phanerogamæ	135	3454	20	102
Cupuliferæ	..	12	..	10
Ericineæ	..	23	..	24
Proportion of trees and plants	$\left\{\begin{matrix}333\\3121\end{matrix}\right\}$ = 1 : 10		$\left\{\begin{matrix}94\\9\end{matrix}\right\}$ = 10 : 1	

Amber is never found isolated in large or small masses in the bituminous wood of the Brown-coal with resin-ducts of a single row of cells, which never contain yellow masses of resin, but only dark-brown transparent resin-drops, as in the *Cupressineæ* or the *Cupressinoxylon*, of Goeppert. The compound resin-ducts of the *Abietineæ* alone are filled with amber.

It is probable that the amber and its plant-remains have been drifted to the places in which they are now found. The author knows of no well-authenticated instances of the occurrence of amber in the Brown-coal formation itself; it occurs in the drift-beds above it, where, how-

ever, it does not appear to have originally belonged. Scheerer has found it in Norway; Von Brevern, at Gischiginsk in Kamtschatka; Rink, in Haven Island, near Disco Island, Greenland; and in these instances it is generally in drift-beds. The supposition, however, that it belongs to the Drift-period is difficult to substantiate, the flora of that period being as yet but little known. The stomach of the fossil Mastodon found in New Jersey contained twigs of *Thuja occidentalis* (found in the Amber-flora); and in the Erie Canal in New York State, at a depth of 118 feet there have been found freshwater shells, together with portions of *Abies Canadensis*, which still grows in the neighborhood, and leaves of which are still recognised (though with some doubt) in the amber. The fossil wood of the Drift-beds of Siberia, also, is nearly related to that of the present day.*

The height at which amber is found at the Castle on the Riesengebirge near Helmsdorf is nearly 1250 feet [German] above the sea level, and at Grossman's Factory near Tannhausen, at 1350 feet.

The amber is not derived from one species of wood only (*Pinites succinifer*), as Professor Goeppert formerly thought, but also from eight other species, including the *Pinus Rinkianus*, in which Vaupelt observed the amber of Disco Island.

It is probable that all the *Abietineæ*, and perhaps the *Cupressineæ*, have furnished their share of the resinous matter (at first consisting of various specifically different resins) that afterwards by fossilization becomes amber; and this is supported by the author's experiments in the formation of amber from resin by the wet process, as in his experiments on the formation of coal from recent plants.†

In form the amber is either like drops, indicative of a former semifluid condition, or as the casts of resin-ducts and cavities. Large nodular masses occur, which must have been accumulated in the lower part of the stem or the root, as in the Copal trees.—(*Quarterly Journal of the Geological Society*, vol. x, No. 37.)

IV. Astronomy.

1. *New Comet*, (Gould's Astron. Journal, Aug. 11.)—A comet was discovered by Klinkerfues, at Göttingen, June 4, 1854. The following elements of its orbit were furnished by Prof. R. Keith, of the Washington Observatory, from observations at Bonn, June 11, and Washington June 26 and July 11.

Perihelion passage,	1854, June 21·7751.	
Longitude of perihelion, - -	273° 6′ 52″·4	Mean eqx.
" " asc. node, - -	347 40 8 ·0	1854·0
Inclination, - - - - -	71 18 46 ·0	
Log. cos. φ, - - - -	8·635714.	
Log. q, - - - - -	9·811593.	
Motion	Retrograde.	

2. *Orbital Elements of Bellona and Amphitrite*, (Comptes Rendus, June 19, 1854.)—The following elements of the new planets *Bellona* and *Amphitrite* are computed by M. Oudemans, of the Observatory of

* See Quart. Journ. Geol. Soc., vol. vi. Part 2. Miscell. p. 66.—Transl.

† Ibid, p. 33.—Transl.

Leyden: the first from observations of March 1 at Bilk, April 12 at Berlin, May 26 at Leyden; the second from observations of March 1, at London, April 11 at Berlin, May 31 at Leyden.

Epoch, March 0, m. t. Berlin.

	Bellona.	*Amphitrite.*	
Mean anomaly, . .	39° 19′ 3″·13	127° 23′ 56″·9	
Longitude of perihelion,	117 23 5 ·6	54 4 26 ·2	M. eqx.
" " asc. node,	144 57 56·3	356 15 54 ·6	1854·0
Inclination, . .	9 27 16·15	6 4 6 ·35	
Angle (sin = excen.) .	9 53 4·50	3 55 43·	
Log. mean daily motion,	2·882097	2·941143	
" semi-axis major,	0·445273	0·405909	

V. Miscellaneous Intelligence.

1. *New Process for Desulphurizing Metals.*—Dr. Homer Holland has presented a process for separating gold from ores of copper, iron, &c., with which it is found associated. It consists in the employment of nitrate of soda with heat. The ores in a state of fine division are mingled with pulverized nitrate of soda in atomic proportion to the sulphur of the ores. The reaction is between two atoms of oxygen from the salt and the sulphur to form sulphurous acid, which may be saved for making SO_3 in the ordinary chambers. The nitrogen escapes as an effete product, and sulphate of soda with sulphate of copper, oxyd of iron and metallic gold remain in the dry mass. A very gentle heat suffices to bring on the action. The resulting dry mass soon falls to powder under the influence of the air, and is lixivated to separate the soluble sulphates. The copper may be separated by the action of scrap iron, or if very abundant may be crystallized from the mother liquor as blue vitriol. The gold is left in the lixivium by this process in a condition remarkably adapted to speedy and perfect amalgamation by mercury, for which purpose any of the machines now in use may be employed. The application of this process upon a large scale will depend upon the ability to procure an ample supply of crude nitrate of soda, abundance of which is said to exist in the province of Iquique.* Dr. O. M. Lieber, who has published a report upon Dr. Holland's process, states that by it he obtained quantities of gold from the tailings of the best amalgamators, greater than had been previously separated from the entire ore. It is well known that the sulphurets of the North Carolina gold-bearing quartz are all auriferous, but the gold exists in such a state, whether mechanical or chemical, is perhaps hardly settled, that it cannot be removed by the ordinary use of mercury. In such cases Dr. Holland's process seems to be peculiarly valuable. Its use for ordinary copper ores appears to be less promising, owing to the present cost of nitrate of soda.

2. *Separation of Nickel and Cobalt by Wöhler's process*, (communicated by Francis E. Dakin, Albany University.)—Having a solution of nickel and cobalt in hydrochloric acid, precipitate by potassa, filter, redissolve in cyanid of potassium, add peroxyd of mercury, triturated in water, little by little till in excess, boil half an hour, filter, wash, dry

* See John H. Blake, this Journal, [1], vol. xliv, p. 1.

and ignite till all the mercury as well as the cyanogen is expelled, leaving pure oxyd of nickel. Precipitate the cobalt by hydrochloric acid, filter, wash, dry and ignite as before, and thus obtain the oxyd of cobalt.

3. *Researches on the Tides of the White Sea.* 2d Memoire; by M. Talyzine, (L'Institut, No. 1044, Jan. 4, 1854, p. 7, St. Petersburg Ac. Sciences.)—In this Second memoir, the author applies himself chiefly to the rise and fall of water during the flood and ebb in the River Kuja, and indicates the mode of observation he adopted. By comparing observations and seeking to unite them by a formula, he has found that the oscillation of the water of the river is composed of three partial oscillations. The first of these has a period equal to that of the flood and the ebb; it may have a height of 30 inches. He calls it the semi-diurnal oscillation. The second has a period which is only the half that of the flood and ebb, it has a height of 5·8 inches. The third has only the third of the period of flood and ebb; it has a height of 3·8 inches. The high waters of the 3d oscillation happen later than those of the 1st by a very considerable quantity, whilst those of the 1st and 3d almost coincide. On tracing the curve representing the rise of water, a period of inflection is seen to belong to this line, which seems to indicate that in the phases of the tide which correspond to it, the water rises more slowly than elsewhere. But this inflection represents the phenomena, called (*manicha*) the stand, and which consists in this, that near the middle of the tide-wave the water ceases to rise and after remaining for some time at the same height, slowly falls. These phenomena are always attended by another which forms an exception to the ordinary flood and ebb. It is known in fact, that between headlands and at the mouths of rivers, the ebb lasts longer than the flood. This also takes place in the White Sea, in localities where there is no stand (manicha). But in localities where it exists the contrary takes place, the flood lasts longer than the ebb, and in the river Kuja this period is an entire hour. It is the 2d and 3d oscillations which cause this predominance of flood over ebb, and M. Talyzine seeks to decompose these influences. On examining the curves of projection, it appears that *the stand* should depend on the second oscillation; in fact, if the high waters of the 2d come later than those of the 1st there ought to be a stand, and if they come sooner this phenomenon should vanish. The examination of the curve of the 3d oscillation seems also to indicate a slackening in the rise of the water, but this delay differs from the stand, because the duration of flood equals that of ebb, for this oscillation has no influence on this duration. Finally by combining the curves of these two oscillations, it appears that the 3d increases the inflection of the 2d, that is, the delay in the falling of the water. From all these considerations, it results that the stand arises from the high waters of the 2d oscillation coming later than those of the 1st, and that the 3d increases the stand produced by the 2d. E. B. H.

4 *Application of the electric telegraph to the determination of the difference of longitude.*—On Nov. 25, operations were commenced for determining the difference of longitude between Brussells and Greenwich by electric telegraph. Three or four days sufficed for connecting the Brussells Observatory with the central station of that city, and to es-

tablish immediate correspondence by this station with the Royal Observatory of England. Some details on this subject follow.

Signals were made every evening between 10^h and 11^h, amounting to about 150 per hour. The first portion of these operations needed not to extend beyond three days; but as the state of the sky, especially in England, did not permit the making of the requisite astronomical observations for properly regulating the clock rates, a condition absolutely essential in this delicate enterprise, the operations were necssarily continued, and even probably prolonged until the evening of December 4, so as to give over 1200 signals. The astronomical observations at Brussells were very numerous, but while in that city the weather was magnificent, the sky at Greenwich was almost constantly overcast. M. Quetelet sent thither M. Bouvy, one of his aids, while Mr. Airy, Astronomer Royal of England, sent Mr. Durakin, one of his aids, to Brussells. The two aids are now to return to their respective posts, and will recommence a new series of electric signals and astronomical observations to eliminate the effects of *personal equations*. Every night, precisely at ten, Brussells mean time, four signals given at three seconds interval, announce that the English observers are at their post. Thirty seconds after, four like signals sent from Brussells, also announce the presence of the Belgian observers. At $10^h\ 1^m$, Greenwich begins to give signals at 10 or 16 seconds interval, but first making known the number of stars observed during the evening by as many 3^s interval beats. Brussells operates then in like manner from $10\frac{1}{4}^h$ to $10\frac{1}{2}^h$, when Greenwich resumes, and then Brussells at $10\frac{3}{4}^h$. The signals have never seemed to lose any of their intensity, in spite of the distance between Greenwich and Brussells; and considering the extraordinary velocity of the galvanic current, we may say that the needles of two galvanometers, placed in the two observatories, traverse simultaneously, and move parallel to each other.

M. Quetelet thought that in his communication to the Academy he ought to limit himself to these simple indications, leaving to Mr. Airy the care of discussing and publishing hereafter the aggregate of the observations undertaken at his request. E. B. H.

5. *Paris Observatory.*—Since the decree of reorganization of the Paris Observatory, several official acts of different dates, the last being March 2, have provided for two places as Astronomers, three as Adjunct Astronomers and three of Astronomical pupils. The following is the present composition of the Observatory personnel.

Director—M. Le Verrier.

Astronomers—M. Faye, M. Yvon-Villarceau.

Adjunct Astronomers—M. Babinet, M. Emile Goujon, M. Chacornac. M. Babinet also is charged with the meteorological observations.

Astronomical Elèves—M. Butillon, M. Reboul, M. Liais. E. B. H.

6. *On manufactured Sea-Water for the Aquarium;* by P. H. Gosse, A.L.S., (Ann. Mag. Nat. Hist., [2], xiv, 65.)—The inconvenience, delay and expense attendant upon the procuring of sea-water, from the coast or from the ocean, I had long ago felt to be a great difficulty in the way of a general adoption of the Marine Aquarium. Even in London it is an awkward and precarious matter; how much more in inland towns and country places, where it must always prove not only

a hindrance, but to the many an insuperable objection. The thought had occurred to me, that, as the constituents of sea-water are known, it might be practicable to manufacture it; since all that seemed necessary was to bring together the salts in proper proportion, and add pure water till the solution was of the proper specific gravity. Several scientific friends to whom I mentioned my thoughts, expressed their doubts of the possibility of the manufacture; and one or two went so far as to say that it had been tried, but that it had been found not to answer; that though it looked like sea-water, tasted, smelt, like the right thing, yet it would not support animal life. Still, I could not help saying, with the lawyers, "If not, why not?"

Experientia docet. I determined to try the matter for myself.

I took Schweitzer's analysis; but as I found that there was some slight difference between his and Laurent's I concluded that a very minute accuracy was not indispensable. Schweitzer gives the following analysis of 1000 grains of sea-water taken off Brighton:

Water, - - - - - - - -	964·744
Chlorid of sodium, - - - - -	27·059
Chlorid of magnesium, - - - -	3·666
Chlorid of potassium, - - - - -	0·765
Bromid of magnesium, - - - -	0·029
Sulphate of magnesia, - - - -	2·295
Sulphate of lime, - - - - -	1·407
Carbonate of lime, - - - - -	0·033
	999·998

The bromid of magnesium and the carbonate of lime I thought I might neglect, from the minuteness of their quantities; as also because the former was not found at all by M. Laurent in the water of the Mediterranean; and the latter might be found in sufficient abundance in the fragments of shell, coral, and calcareous algæ, thrown in to make the bottom of the aquarium. The sulphate of lime (plaster of Paris) also I ventured to eliminate, on account of its extreme insolubility, and because M. Laurent finds it in excessively minute quantity. The component salts were then reduced to four, which I used in the following quantities:

Common table salt, - - -	3½ ounces.	
Epsom salts, - - - -	¼ "	
Chlorid of magnesium, - - -	200 grains	Troy.
Chlorid of potassium, - - -	40 "	

To these salts, thrown into a jar, a little less than four quarts of water (New River) were added, so that the solution was of that density that a specific gravity bubble 1026 would just sink in it.

The cost of the substances was—sulph. mag. 1*d.*; chlor. mag. 3*d.*; chlor. pot. 1¼*d.*; salt, nil;—total, 5¼*d.* per gallon. Of course if a larger quantity were made, the cost of the materials would be diminished, so that we may set down 5*d.* per gallon as the maximum cost of sea-water thus made. The trouble is nothing, and no professional skill is requisite.

My manufacture was made on the 21st of April. The following day I poured off about half of the quantity made (filtering it through

a sponge in a glass funnel) into a confectioner's show-glass. I put in a bottom of small shore-pebbles, well washed in fresh water, and one or two fragments of stone with fronds of green sea-weed *(Ulva latissima)* growing thereon. I would not at once venture upon the admission of animals, as I wished the water to be first somewhat impregnated with the scattered spores of the *Ulva;* and I thought that if any subtle elements were thrown off from growing vegetables, the water should have the advantage of it, before the entrance of animal life. This too is the order of nature; plants first; then animals.

A coating of the green spores was soon deposited on the sides of the glass, and bubbles of oxygen were copiously thrown off every day under the excitement of the sun's light. After a week therefore I ventured to put in animals as follows:

2 *Actinia mesembryanthemum.*	*Coryne ramosa.*
7 *Serpula triquetra.*	*Crisia eburnea.*
3 *Balanus balanoides.*	—— *aculeata.*
2 *Sabella* —— ?	*Cellepora pumicosa.*
2 *Sabellaria (alveolata?)*	*Cellularia ciliata.*
2 *Spio vulgaris.*	*Bowerbankia imbricata.*
1 *Cynthia (quadrangularis?)*	*Pedicellina Belgica.*

These throve and flourished from day to day, manifesting the highest health and vigor; the plants (including one or two Red Weeds that were introduced with the animals) looked well, and the water continued brilliantly crystalline. Within the succeeding month specimens of *Actinia mesembryanthemum*, *A. anguicoma* and *A. clavata*, a *Trochus umbilicatus*, and a *Littorina littorea* were at different times added.

Six weeks have now elapsed since the introduction of the animals. I have just carefully searched over the jar, as well as I could do it without disturbing the contents. I find every one of the species and specimens mentioned above, all in high health; with the exception of some of the Polyzoa, viz. *Crisia aculeata*, *Cellepora pumicosa*, *Cellularia ciliata*, and *Pedicellina Belgica*. These I cannot find, and I therefore conclude that they have died out; though if I chose to disturb the stones and weeds, I might possibly detect them. These trifling defalcations do in no wise interfere with the conclusion, that the experiment of manufacturing sea-water for the Aquarium has been perfectly successful. [Lime is important to many animals and had better be added.]

7. *Observations, economical and sanatory, on the employment of Chemical Light for artificial Illumination;* by Dr. E. Frankland, F.C.S., (Proc. Roy. Inst. of Gr. Britain, 1853, 319.)—There are two principal sources of artificial light, viz., electricity and the chemical force; the latter, however, has been, and still is, the only practical source of all artificial light. Although light can be thus obtained by the chemical action of substances belonging to all three kingdoms, yet closer observation demonstrates that the illuminating effect obtained from animal and mineral bodies is primarily derived from the vegetable kingdom; every plant being an apparatus for the absorption and concentration of light and heat from the solar rays, and for the retention of these forces during its passage through the subsequent stages in the formation of vegetable fuel.

Until the commencement of the present century artificial light was derived almost exclusively from the animal kingdom; but the great economy attending its immediate production from our vast stores of vegetable fuel is becoming more and more apparent, and is in fact so generally admitted, as to render more than a mere allusion to it and a glance at the following Table, unnecessary.

TABLE—showing the comparative cost of light from various sources, each equal to 20 sperm candles burning 120 grains per hour each, for 10 hours.

	s.	d.
Wax,	7	2½
Spermaceti,	6	8
Tallow	2	8
Sperm oil (Carcel's Lamp),	1	10
London gases, B, C, D, E,*	0	4¼
Manchester gas,	0	3
London gas, A,	0	2¼

We will therefore confine our attention principally to the light produced from vegetable fuel, in considering the economical and sanatory bearings of artificial light.

The production of artificial light depends upon the fact, that at certain high temperatures all matter becomes luminous. The higher the temperature the greater is the intensity of the light emitted. The heat required to render matter luminous in its three states of aggregation, differs greatly. Thus, solids are sometimes luminous at comparatively low temperatures, as phosphorus and phosphoric acids. (A jet of flame produced by the formation of these substances was exhibited, and its temperature shown to be inadequate to the ignition or even scorching of the finest cambric or gun cotton.) Usually, however, solids require a temperature of 600° or 700° F. to render them luminous in the dark, and must be heated to 1000° F. before their luminosity becomes visible in daylight. Liquids require about the same temperature. But to render gases luminous, they must be exposed to an immensely higher temperature; even the intense heat generated by the oxy-hydrogen blowpipe scarcely suffices to render the aqueous vapor produced visibly luminous, although solids, such as lime, emit light of the most dazzling splendor when they are heated in this flame. Hence, those gases and vapors only can illuminate, which produce or deposit solid or liquid matter during their combustion. This dependence of light upon the production of solid matter is strikingly seen in the case of phosphorus, which when burnt in chlorine produces a light scarcely visible; but when consumed in air or oxygen, emits light of intense brilliancy: in the former case the *vapor* of chlorid of phosphorus is produced, in the latter *solid* phosphoric acid.

Several gases and vapors possess this property of depositing solid matter during combustion, but a few of the combinations of carbon and hydrogen are the only ones capable of practical application: these latter compounds evolve during combustion only the same products as those

* London gases, A, B, C, D, E.—These are the gases furnished to consumers by five of the principal London Companies. For obvious reasons the names of the Companies are not mentioned.

generated in the respiratory process of animals, viz. carbonic acid and water. The solid particles of carbon which they deposit in the interior of the flame, and which are the source of light, are entirely consumed on arriving at its outer boundary; their use as sources of artificial light under proper regulations is therefore quite compatible with the most stringent sanatory rules.

In the usual process of gas manufacture there are generated in addition to these illuminating hydrocarbons two other classes of gaseous constituents, viz. impurities and diluents. With the exception of bisulphuret of carbon and some organic compounds containing sulphur, all the impurities are removed in the usual processes of purification, which have now been brought to great perfection; but the presence of these sulphur compounds in coal gas is very objectionable, and constitutes the chief barrier to the universal employment of gas in dwelling-houses. The attention of the manufacturer ought therefore now to be earnestly directed to the discovery of means for preventing the formation of these compounds, as it will probably be found impossible to remove them from the gas when once they have been formed.

In addition to traces of these sulphur compounds, purified coal gas contains only the following ingredients:

		Formula.
Illuminating constituents.	Olefiant gas, - - -	C_2H_2
	Propylene? - - - -	C_3H_3
	Butylene? - - - -	C_4H_4
	Other hydrocarbons, - -	unknown.
Diluents.	Light carburetted hydrogen, -	CH_2*
	Hydrogen, - - - -	H
	Carbonic oxyd, - - -	CO

The light emitted during the combustion of coal gas is due entirely to the first or illuminating class of constituents, which yield an amount of light proportional to the quantity of carbon contained in a given volume; thus, propylene and butylene yield respectively 50 and 100 per cent. more light than olefiant gas, because they contain respectively 50 and 100 per cent. more carbon in a given volume.

It would not be desirable to employ a gas containing only luminiferous ingredients, even if it were possible to manufacture such a gas, because it is exceedingly difficult to consume these constituents without the production of smoke attendant on imperfect combustion. A diluting material is therefore necessary to give the flame a sufficient volume, so as to separate the particles of carbon farther asunder, and thus diminish the risk of their imperfect combustion.

All the three diluents above mentioned perform this office equally well; but if we study their behavior during combustion we shall find that in a sanatory point of view, hydrogen is greatly to be preferred.

The two objections most frequently urged against the use of gas in apartments are, first the heat which it communicates to the atmosphere, and, second, the deterioration of the air by the production of carbonic

* This gas has usually been described as possessing a certain amount of illuminating power, but a specimen of it brought from the coal strata beneath Chat Moss, Lancashire, showed that it yields no more light than hydrogen or carbonic oxyd when consumed from a *fish-tail* burner.

acid. Now, in their action upon the atmosphere in which they are consumed, the above three diluents present striking differences in these two respects.

One cubic foot of light carburetted hydrogen, at 60° F. and 30 in. barometrical pressure, consumes two cubic feet of oxygen during its combustion, and generates one cubic foot of carbonic acid, yielding a quantity of heat capable of heating 5 lbs. 14 oz. of water from 32° to 212°, or causing a rise of temperature from 60° to 80°·8 in a room containing 2,500 cubic feet of air.

One cubic foot of carbonic oxyd at the same temperature and pressure, consumes during combustion ½ a cubic foot of oxygen, generates one cubic foot of carbonic acid, and affords heat capable of raising the temperature of 1 lb. 14 oz. of water from 60° to 66°·6.

One cubic foot of hydrogen, at the same temperature and pressure, consumes ½ a cubic foot of oxygen, generates no carbonic acid, and yields heat capable of raising the temperature of 1 lb. 13 oz. of water from 32° to 212°, or that of 2,500 cubic feet of air from 60° to 66°·4.

This comparison shows the great advantage which hydrogen possesses over the other diluents, especially over light carburetted hydrogen, which is evidently a very objectionable constituent, and shows that a normal gas for illuminating purposes should consist of illuminating hydrocarbons diluted with pure hydrogen. No method is known by which a gas of exactly this composition can be manufactured, but a very close approximation has been lately made to this normal gas, by the employment of a process known as White's Hydrocarbon method of gas-making. In this process the very ingenious principle is adopted of generating the illuminating constituents in as concentrated a form as possible in one retort, and the diluents consisting principally of hydrogen free from light carburetted hydrogen in another. By this arrangement the diluents can be employed for a very remarkable and highly interesting purpose; they are conducted through the retort in which the illuminating constituents are being generated, in such a manner as rapidly to sweep out those constituents, before they have time to become decomposed by contact with the red hot interior surfaces of the retort, a mode of destruction which occurs so largely in the usual process of gas-making. This mode of treatment produces a gain in the amount of illuminating power derived from a given weight of coal, equal to from 50 to upwards of 100 per cent., whilst the increase in quantity of gas is frequently 300 per cent.

The gas thus manufactured differs principally from coal-gas made by the ordinary process, in having a large portion of the light carburetted hydrogen replaced by hydrogen; it is therefore in a sanatory point of view the best gas hitherto produced. This is seen in the following Table, which exhibits the amount of carbonic acid and heat generated per hour by various sources of light, each equal to 20 sperm candles burning at the rate of 120 grains of sperm per hour.

	Carbonic acid.	Heat.
Tallow, - - - - -	10·1 cubic feet	100
Wax, - - - - -	8·3 " "	82
Spermaceti, - - - -		
Sperm oil (Carcel's Lamp),	6·4 " "	63
London gases, B, C, D, E,	5·0 " "	47

	Carbonic acid.	Heat.
Manchester gas, - -	4·0 cubic feet	32
London gas, A, - -	3·0 " "	22
Boghead hydrocarbon gas,	2·6 " "	19
Lesmahago hydrocarbon gas,	2·5 " "	19

Notwithstanding the great economy and convenience attending the use of gas, and in a sanatory point of view, the high position which, as an illuminating agent, coal-gas of proper composition occupies, its use in dwelling houses is still extensively objected to. The objections are partly well founded and partly groundless. As is evident from the foregoing table, even the worst London gases produce, for a given amount of light, less carbonic acid and heat than either lamps or candles. But then, where gas is used, the consumer is never satisfied with a light equal in brilliancy only to that of lamps or candles, and consequently, when three or four times the amount of light is produced from a gas of bad composition, the heat and atmospheric deterioration greatly exceed the corresponding effects produced by the other means of illumination. By using a gas however of nearly the normal composition, such as the hydrocarbon gases above named, it is evident that three or four times the light may be employed, with the production of no greater heat or atmospheric deterioration, than that caused by wax candles or the best constructed oil lamps.

But there is nevertheless a real objection to the employment of gas-light in apartments, founded upon the production of sulphurous acid during its combustion: this sulphurous acid is derived from bisulphuret of carbon, and the organic sulphur compounds, which have already been referred to as incapable of removal from the gas by the present methods of purification.

The formation of sulphurous acid can readily be proved and even its amount estimated, by passing the products of combustion of a jet of gas through a small Liebig's condenser; the condensed product being heated to boiling with the addition of a few drops of nitric acid, and then treated with solution of chlorid of barium, yields a white precipitate of sulphate of baryta, if any sulphur compound be present in the gas.

These impurities, which are encountered in almost all coal-gas now used, are the principal if not the only source of the unpleasant symptoms experienced by many sensitive persons, in rooms lighted with gas. It is also owing to the sulphurous acid generated during the combustion of these impurities, that the use of gas is found to injure the bindings of books, and impair or destroy the delicate colors of tapestry. Therefore the production of gas free from these noxious sulphur compounds is at the present moment a problem of the highest importance to the gas manufacturer, and one which demands his earnest attention.

As it is nearly impossible for the consumer to procure gas free from these objectionable compounds, the only method of obviating their unpleasant and noxious effects is to remove entirely the products of combustion from the apartments in which the gas is consumed, and thus prevent them from mingling with the circumambient air. This suggestion was first made by Faraday, who, for accomplishing this object, contrived the very beautiful and effective ventilating burner exhibited in operation upon the lecture table. This apparatus, which is used at Buckingham Palace, Windsor Castle, the House of Peers, and in many

public buildings, may be truly said to have brought gas illumination to perfection; for not only are all the products of combustion conveyed at once into the open air, but nearly the whole of the heat is in like manner prevented from communicating itself to the atmosphere of the room. The only obstacles to the universal adoption of this description of burner are its expense, and the difficulty of conveying the ventilating tube safely into the nearest flue without injuring the architectural appearance of the room. The public at large will therefore still await the removal of the objectionable compounds in question, by the gas manufacturer, before they will universally adopt this otherwise delightful means of artificial illumination. E. F.

8. *Mechanical Action of Heat;* by F. A. P. Barnard.—*Prof. Dana:*—An apology is perhaps due from me to Mr. Rankine, for attributing exclusively to another, in my article on "Heated Air," sent you in November last, but published in March, a formula of thermo-dynamics which was first certainly presented by him in its most complete form, and which is as beautiful for its simplicity as it is practically useful. It may explain if not excuse my inadvertance, to remark that I but followed Mr. Joule in this respect, whose article on the "Economical Production of Mechanical Effect from Chemical Forces," republished from the Memoirs of the Lit. and Phil. Soc. of Manchester in the Lond. Edin. and Dublin Phil. Mag., for Jan. 1853, was necessarily referred to in the preparation of mine; and had been commented upon by Mr. Rankine himself in a letter to Mr. Joule, without his having noticed the error. Not having access to a complete series of the Transactions of the R. S. of Edinburgh, I had been obliged to send abroad for the volume containing Mr. Rankine's original paper, and had not received it, when my article was forwarded to you. Prof. Thompson's paper I had however read in the Lond., Edin. and Dublin Phil. Mag. for July, August and September, 1852; and also his demonstration of the formula in question in the Lond. Phil. Trans., Part I, of the same year. In the paper first mentioned, the formula is not only deducible from the expression cited by Mr. Rankine in the last number of this Journal,* viz.,

$$\frac{W}{H}=1-\varepsilon^{-\frac{1}{J}\int_{\tau_{,}}^{\tau''}\mu d\tau};$$

but it is actually *deduced*, by combining with the foregoing Mr. Joule's suggestion that "Carnot's Function" may vary inversely as the absolute temperatures, in expression (12) of the same article; which is, when W and H are taken as above to represent the total motive power gained, and the mechanical equivalent of the total heat expended, severally,

$$\frac{W}{H}=\frac{S-T}{\frac{1}{E}+S};$$

where S and T are the thermometric temperatures of the source of heat and of the refrigerator respectively, and E is the reciprocal of the number of degrees between the absolute and the thermometric zero. It is evident that this, when the symbols employed by me to denote

* There is a typographical error in this expression as given in the last number of this Journal.

absolute temperatures are substituted, becomes

$$\frac{W}{H} = \frac{\tau' - \tau_1}{\tau'}$$

which is the formula ascribed by me to Prof. Thompson.

A foot note, however, by the writer of the paper, had I sufficiently attended to it, would have informed me that Mr. Rankine was independently in possession of substantially the same formula as early if not earlier; for Prof. Thompson remarks that Mr. Rankine has communicated to him a formula for the ratio of power obtained to the mechanical equivalent of the heat expended, which agrees exactly with his own.

It may be remarked also, that Mr. Rankine had published his formula in the Lond. and Edin. Phil. Mag. as early as July, 1851, in a note to his letter to Poggendorff; and had reproduced it in the same journal in February and in June, 1853; so that I could hardly be unaware that he had independently originated it.

I am happy, in making this correction, to express my admiration of the ingenious theory suggested and so ably developed by Mr. Rankine, to account for the mechanical phenomena of heat. I regard the coincidence of the results deduced from premises so different as those employed by himself and Prof. Thompson, as one of the most beautiful examples of the consistency of truth which the history of science furnishes.

The necessity of a negative constant, represented by $\varkappa$ in the denominator of the formula as given by Mr. Rankine, for temperatures determined by the air thermometer, was pointed out by him alone. This constant, which becomes zero on supposition of the rigorous conformity of elastic fluid, at all temperatures, to "the gaseous laws," and which is always small, was neglected by me, partly because I took the formula directly from Prof. Thompson (Lond. Phil. Trans., Part I, 1852), and partly because Mr. Rankine had shown it to be too inconsiderable to be important to the purpose I had in hand.

Univ. of Alabama, July 7, 1854.

9. *Siluria, The History of the Oldest Known Rocks containing Organic Remains, with a brief sketch of the Distribution of Gold over the Earth;* by Sir R. I. MURCHISON, G. C. St. S., &c. &c. 524 pp. 8vo, with 37 plates of fossils. London, 1854. J. Murray.—A work of great value and interest.

10. *Elementary Chemistry, Theoretical and Practical;* by GEORGE FOWNES, F.R.S., late Professor of Practical Chemistry in University College, London. Edited with additions by ROBERT BRIDGES, M.D., Prof. Chem. Philad. Coll. of Pharmacy, etc. A new American from the last London edition, with numerous illustrations on wood. 555 pp. 12mo. Philadelphia, 1853. Blanchard & Lea.—This is a most excellent text-book for class instruction in chemistry, whether for schools or colleges. The death of Mr. Fownes left the recent revision of the work to the able chemists, H. Bence Jones and A. W. Hofmann of London, under whose auspices the last (4th) London edition was prepared for the press.

11. *Manual of Natural History for the use of Travellers*, being a Description of the Families of the Animal and Vegetable Kingdoms, with Remarks on the Practical Study of Geology and Meteorology; to which are appended Directions for collecting and preserving. By AR-

THUR ADAMS, M.R.C.S., F.L.S., &c., WILLIAM DALFOUR BAILIE, M.D., F.R.S.E., and CHARLES BARRON, Curator of the Royal Naval Museum at Haslar. 750 pp. 16mo. London, 1854. John Van Voorst.

12. *The Microscope and its application to Chemical Medicine;* by LOVELL BEALE, M.B. London, Prof. of Physiology and General and Morbid Anatomy in King's College, London. 302 pp. 12mo. *London*, 1854. Samuel Highley.—The value of the microscope to the medical man is beginning to be understood and appreciated. The Treatise of Prof. Beale is well calculated to instruct the student in the use of the instrument and in the methods of dissecting, preparing, and examining objects that may be of interest to him. After earlier elementary information, a chapter is devoted to the injecting of preparations; another to the modes of preparing tissues, the kidney, liver, &c.; others to examinations of the brain, nerves, nervous and serous membranes, morbid growths, etc, etc. The details are full and have special reference to the necessities of the medical man. The work is illustrated with a plate of crystalline substances, and numerous fine wood-cuts, among which are two large views of microscopes, and figures of the various tissues, cells, morbid growths or deposits, etc., described in the text.

We observe that in the remarks on the application of a goniometer to the microscope, it states that "the simplest method of measuring the angles of microscopic crystals is that of Schmidt." This Mr. Schmidt, is Prof. J. Lawrence Smith, late of the Virginia University, and now of the Louisville Medical School.

13. *Report on the Geology of the Coast Mountains and part of the Sierra Nevada*, embracing their industrial resources in Agriculture and Mining; by Dr. JOHN B. TRASK. Assembly Doc., No. 9. San Francisco, 1854.—A pamphlet of 96 pages, and not a result of a careful survey.

14. *Natural History Review.* Dublin, April, 1854. 104 pp. 8vo.—This new quarterly is announced to appear on the 1st of January, April, July and October. The April Number contains Reviews of works on Science, with notices of the contents of some prominent scientific periodicals in Zoology and Botany, and the Proceedings of Irish scientific societies. It promises to be a valuable work.

15. *Bibliographical Notices by M. Jerome Nicklès.*

Methode de Chimie par A. LAURENT, avec une notice par M. BIOT. 1 vol. in 8° de 460 pages. Paris, chez Mallet-Bachelier.

Théorie Générale des effets dynamiques de la Chaleur, par F. REECH, Ingenieur de la Marine, Directeur de l'école d'application du génie maritime.—S. Carnot published in 1824, a work entitled "*Reflexions sur la puissance motrice du feu.*" It contained some remarkable views on the manner of producing motive power with heat. The reasoning of Carnot was subjected by M. Clapeyron to mathematical analysis; both calculations and reasoning being founded on the absurdity of admitting the possibility of *creating* motive force or heat. The recent discoveries having deprived us of one of the properties considered fundamental by these authors, some writers, as Joule, Thompson, Rankine, Mayer and Clausius have applied themselves to the fuller investigation of what appeared inaccurate in the relations established by Carnot and Clapeyron.

Persuaded that in all these researches too much importance had been given to hypotheses, the logical train of reasoning of Carnot being lost

sight of, M. Reech has made the subject complete from a new point of view, establishing a formula in harmony alike with the results of M. Joule and those of M. Regnault. The work of M. Reech has excited much attention among mechanicians, and it will be studied with interest by all who are occupied with the mechanical properties of steam, warm air, etc. The volume is a quarto of 212 pages, published by Mallet-Bachelier, Paris.

The following works have also appeared at the same house:

Précis des œuvres mathematiques de Fermat et de l'arithmetique de Diophante par BRASSINE, Prof. à l'école d'artillerie de Toulouse. 1 vol. 8vo.—P. Fermat, inventor of the infinitesimal calculus, founder of the theory of numbers, published his collected memoirs in 1679. These remarkable works having become very rare, government made in 1843 an appropriation for their reprint; this project never having been carried out, M. Brassine has collected the principal works of Fermat, and added the correspondence between Fermat, Pascal, Digby, etc. The volume includes the statements of the propositions which enter into the six books of Diophantus, with the observations of Fermat which are very rich in fine theorems. This portion presents to teachers an excellent collection of arithmetical problems.

—— *Traité du calcul differentiel par l'abbé* LATRENT. 1 vol. 8vo.—This valued work is indispensable to all who are studying the higher mathematics. It serves as an introduction to the treatises of Cauchy, Moigno, and especially the—

—— *Traité de mecanique par* M. DUHAMEL. 1 vol. in 8vo.—M. Duhamel, member of the French Institute, has been for a long time director of the studies of the Polytechnic School, where he still holds the professorship of mechanics with unquestioned ability. His treatise has been very successful among Professors, Engineers, the officers of artillery, and others.

—— *Uranographie ou Traité élémentaire d'astronomie par* FRANCŒUR. 1 vol. in 8vo. 6th edition.—This edition is posthumous, and is published by the son of M. Francœur. The father who was known among scientific men by his vast and varied attainments, was distinguished before the public for his rare talent in exposition. The Uranography partakes of this happy quality. Without requiring other calculations than those of arithmetic, and the elements of geometry, Francœur initiates the reader into the most delicate questions of astronomy, and even sets him to resolving important problems. The author has also in view the instruction of men of letters. Knowing that poets and orators of ancient time were well instructed in Astronomy, Physics, &c., he wished that our modern writers should be equally well taught in science, that they might not absurdly employ themselves with ideas suggested to them, which they did not understand, or like certain lawyers plead for a patent right on some chemical matter which they knew as little of as the judges. The first two parts of the book might be read with profit by all; the third part contains calculations, and is addressed to the learned in the science.

—— *Leçons de Cosmographie par* MM. HARANT et LAFITTE. 1 vol. in 8vo. of 188 pages.—This work has a less general end; it has been got up in view of the new programme of instruction which the government has imposed upon Colleges, Lyceums, and on candidates for ad-

mission to the polytechnic and naval schools, and the school of arts and manufactures. In spite of the narrowing restrictions, the authors have succeeded in giving a truthful sketch of history, and at the same time in delineating the successive developments of the human mind. The clear and methodical expositions which characterize the work, render it valuable to students and even to professors.

—— *Chimie photographique par* MM. BARRESWILL et DAVANNA. 1 vol. in 8vo, 287 pages.—The authors give the theory of photographic manipulations, the photographic processes on metallic plates, on paper dry and moist, upon glass prepared with collodion and with albumen; the manner of preparing the same; of employing the reagents, and of making the residues useful; the newest and most perfect methods; and finally they treat of engraving and lithography. With the aid of this book, the photographer, though little acquainted with chemistry, is enabled to purchase safely, and to employ judiciously, the best materials.

—— *Recherches cliniques sur les Eaux-Bonnes par* E. CAZENAVE. Pamphlet in 8vo. of 100 pages.—The author who is a physician at Eaux-Bonnes (Pyrenees) reports his observations and the results of his investigations on the diseases which belong to the vicinity of those waters. This work is especially interesting to physicians and naturalists.

—— *Des mesures propres à prévenir les collisions sur les chemins de fer, par* M. COUCHE, professeur a l'ecole des Mines de Paris, Pamphlet in 8vo, 48 pages. Paris, chez Carillan-Goeury.

—— *Des accidents sur les chemins de fer par* E. WITH. Pamphlet in 12mo. of 148 pages. Paris, Mallet-Bachelier.—These two pamphlets have been called out by the numerous railroad accidents which have occurred in France during the last year. The authors discuss with much ability the different methods that may be employed to prevent such accidents. M. Couche is chief engineer of a prominent railroad, and therefore well situated to judge of these things; this is true also of M. With.

—— *Stenarithmie ou Abréviation des calculs par* GOSSART, 1 vol. in 12 de 120 pages. Paris, Mallet-Bachelier.

—— *Problème de Mathématiques et de Physique par* DUVIGNAU. 1 vol. in 12 de 140 pages. Paris, Mallet-Bachelier.

—— *Arithmétique a l'usage des instituteurs par Finance*, 1 vol. in 12 de 236 pages. Paris, chez Mallet-Bachelier.

PROCEEDINGS OF THE ACAD. OF NAT. SCI. OF PHILADELPHIA. Vol. VII. No. 3.—p. 72. Bones of Saurians from near Greenville, Clarke Co., Arkansas (Brimosaurus grandis, L., Cimoliasaurus magnus, L., with a plate); *J. Leidy.*—p. 73. Synopsis of the Cucuiides of the United States; *J. L. Le Conte.*—p. 79. Notice of some Coleopterous Insects from the Collections of the Mexican Boundary Commission; *J. L. Le Conte.*—p. 85. Notice of a new species of Salamandra from the Northeastern part of the United States; *C. Girard.*—p. 86. A list of the North American Bufonids, with Diagnoses of new species; *C. Girard.*—p. 89. A bone referred to Sus Americanus by Harlan and named Harlanus by Owen, who made it a tapiroid pachyderm, shown by *Leidy* to belong to a ruminant, and this the *Bison latifrons.*—p. 90. On fossils from South Carolina and Nebraska; *J. Leidy.*—p. 91. Descriptions of new Reptiles from California; *E. Hallowell.*—p. 97. On a genus and species of Serpent from Honduras, pronounced to be new; *E. Hallowell.*—p. 98. On the Geographical Distribution of Reptiles, with descriptions of several species supposed to be new, and corrections of former papers; *E. Hallowell.*—p. 105. Descriptions of four new species of Viviparous Fishes from Sacramento River, and the Bay of San Francisco; *W. P. Gibbons.*—p. 106. Synopsis of the Dermestidæ of the United States; *J. L. Le Conte.*—p. 113. Synopsis of the Byrrhidæ; *J. L. Le Conte.*—p. 118. Descriptions of new Birds collected between Albuquerque, N. M., and San Francisco, California, by Dr. C. B. R. Kennerly and H. B. Möllhausen; *S. F. Baird.*

Plate No. 1

No. 1 Maximum S. (Syzygies)

Type Curves. Key West. Max. Decl. of Moon

Semi-Diurnal Curve (S)

Diurnal Curve (D)

Compound Curve

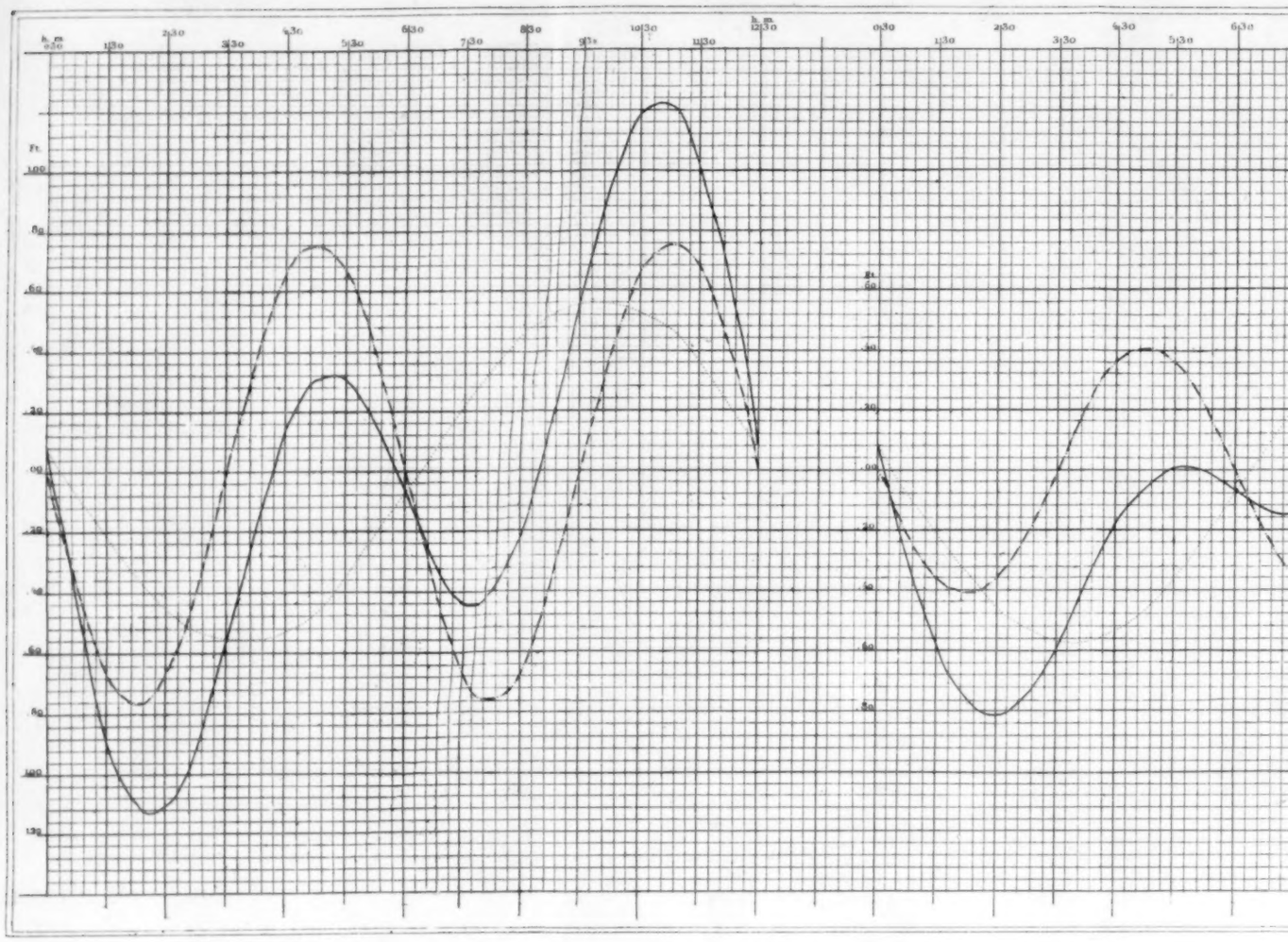


Plate No. 1 (bis)

No. 3. Maximum S. (Syzygies)

Type Curves Key West Zero Decl. of Moon

Semi-Diurnal Curve (S)

Diurnal Curve (D)

Compound Curve

No. 4. Minimum S. (Qua

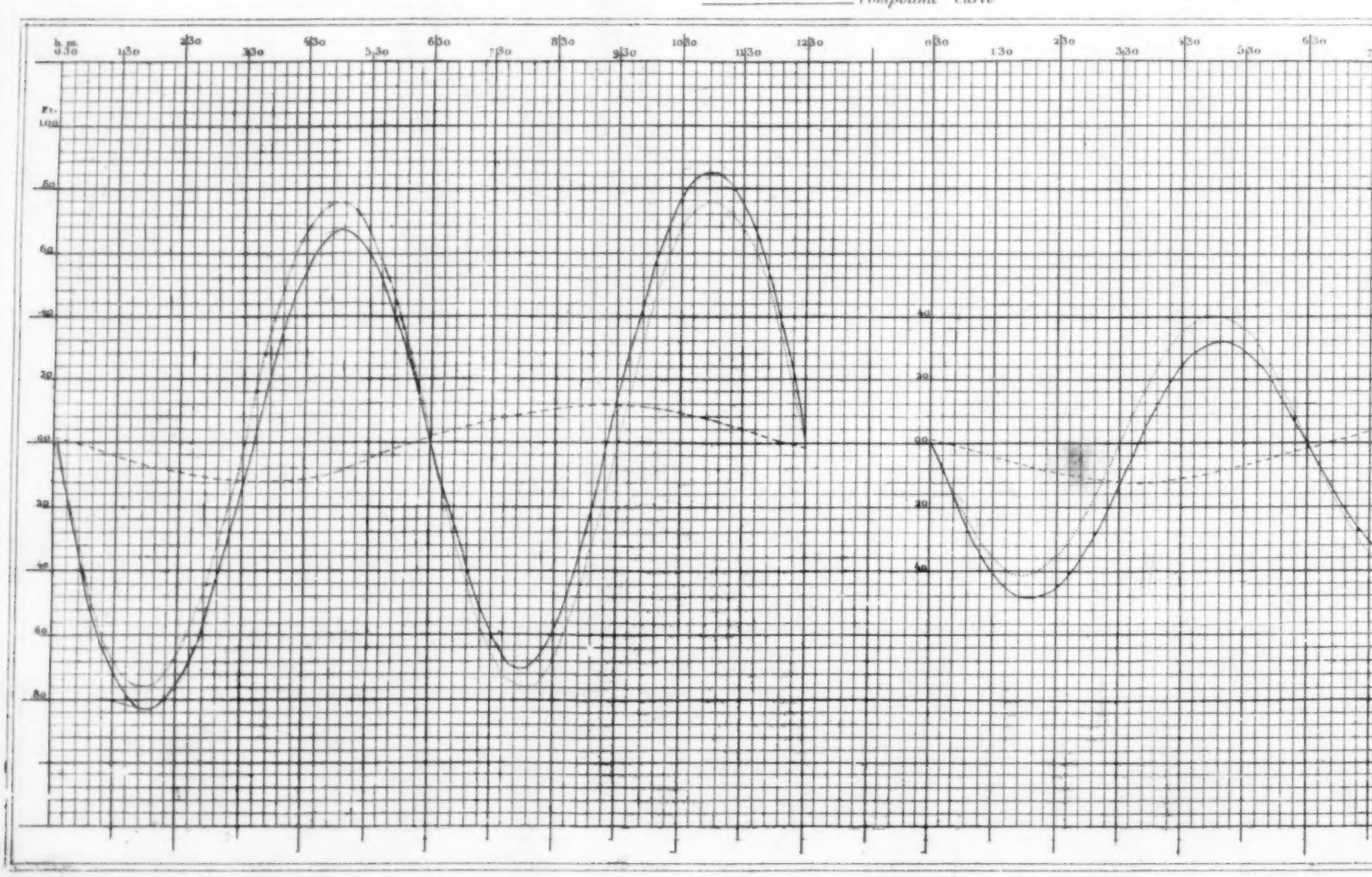
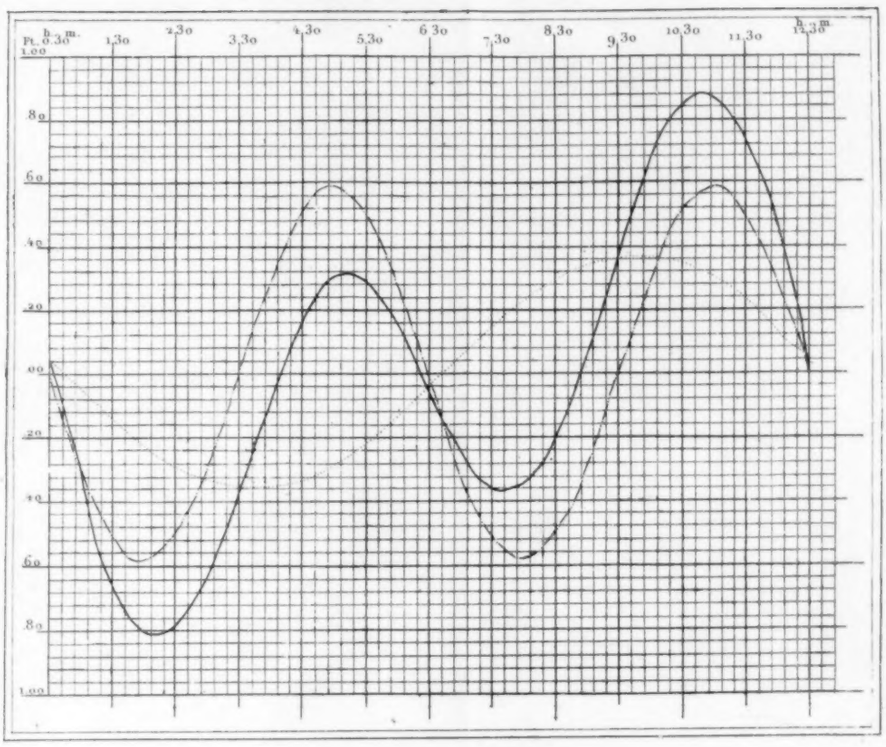


Plate I *(tris)*

Type Curves, Key West. Mean Decl. of Moon

No. 5 *Mean S.* *Mean D.*

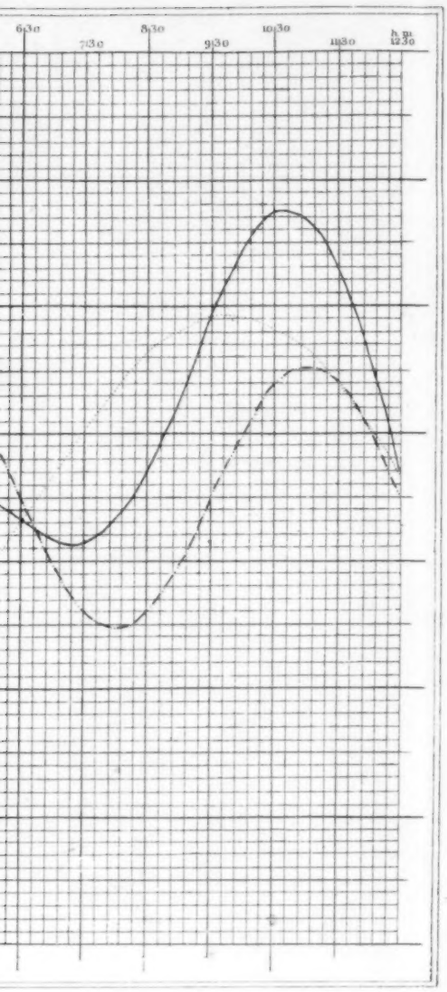


Semi-Diurnal Curve (S)

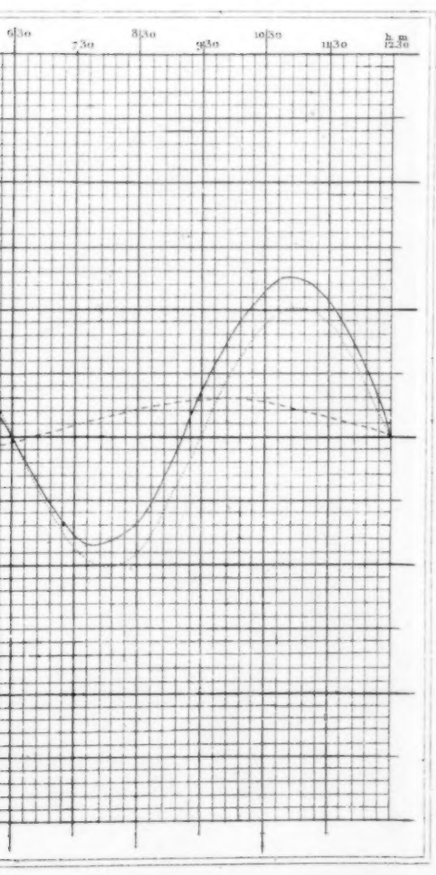
Diurnal Curve (D)

Compound Curve

No. 2 Minimum S. (Quadratures)



S. (Quadratures)



No 28

Plate No. 2. *Comparison of half monthly inequality in interval and height from*

No. 1 Interval

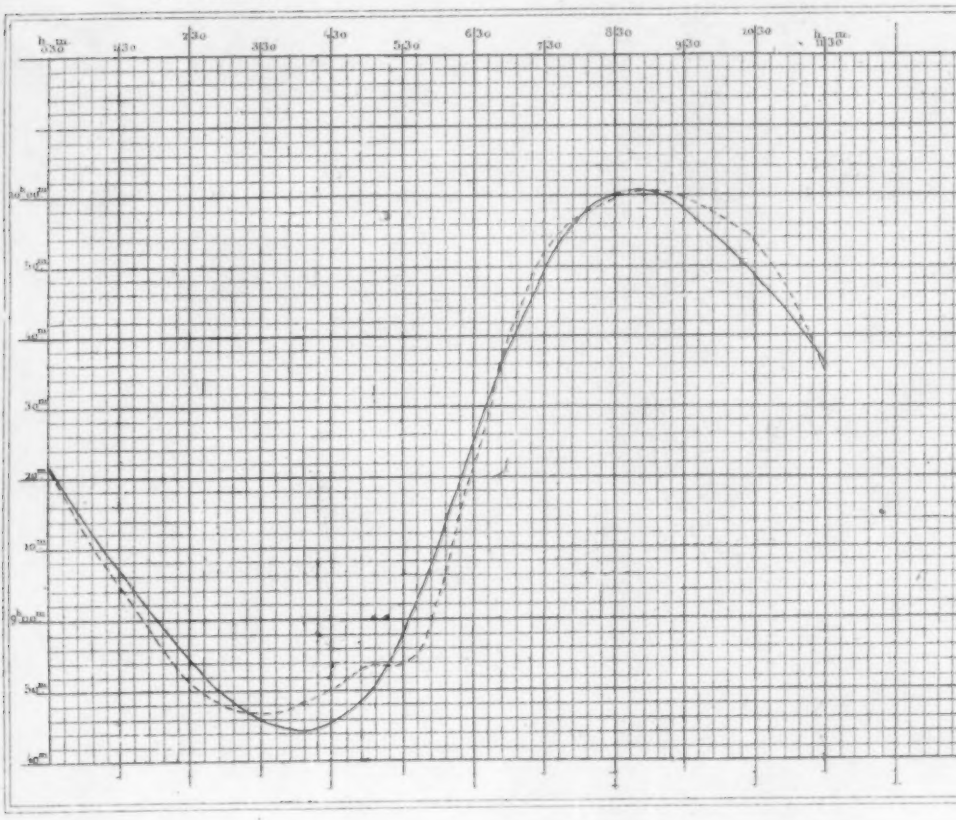
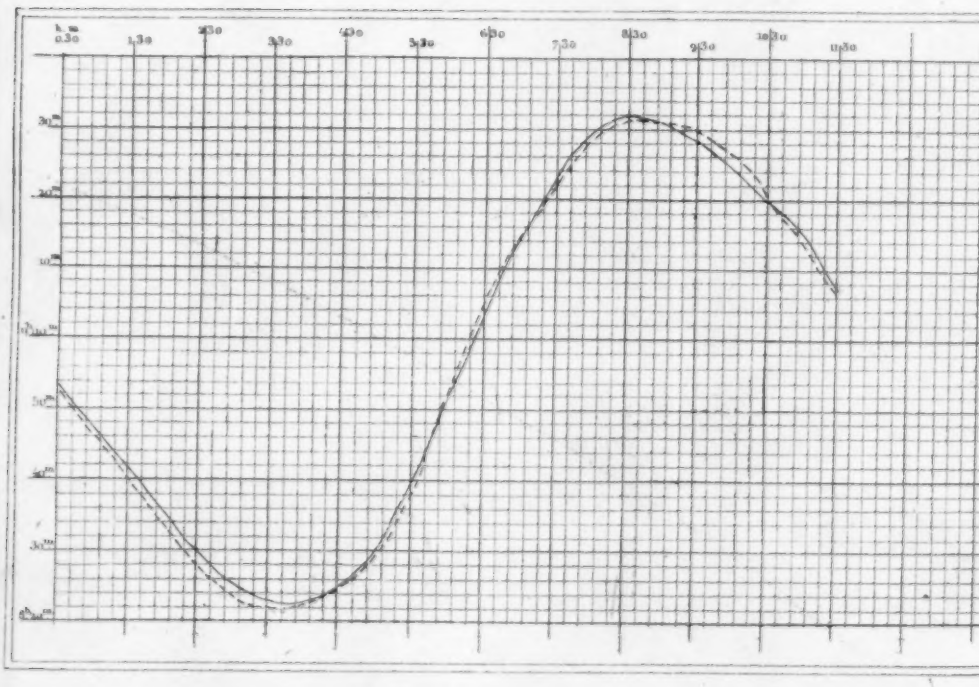


Plate No. 4

Comparison of halfmonthly inequality in Interval and H

No 1 Interval

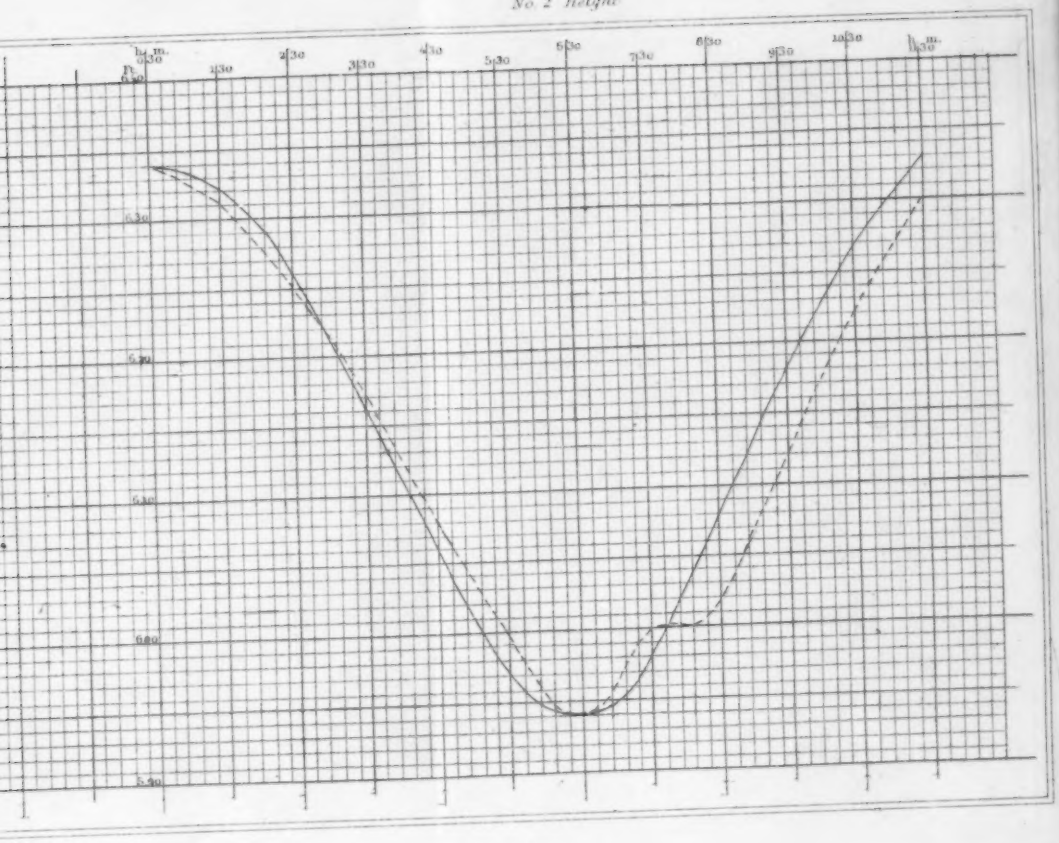


from theory and observation — Whole year

Theory

Observation

No. 2 Height



al and Height from theory & observation — Semi-diurnal wave

Observation

Theory

No. 2 Height

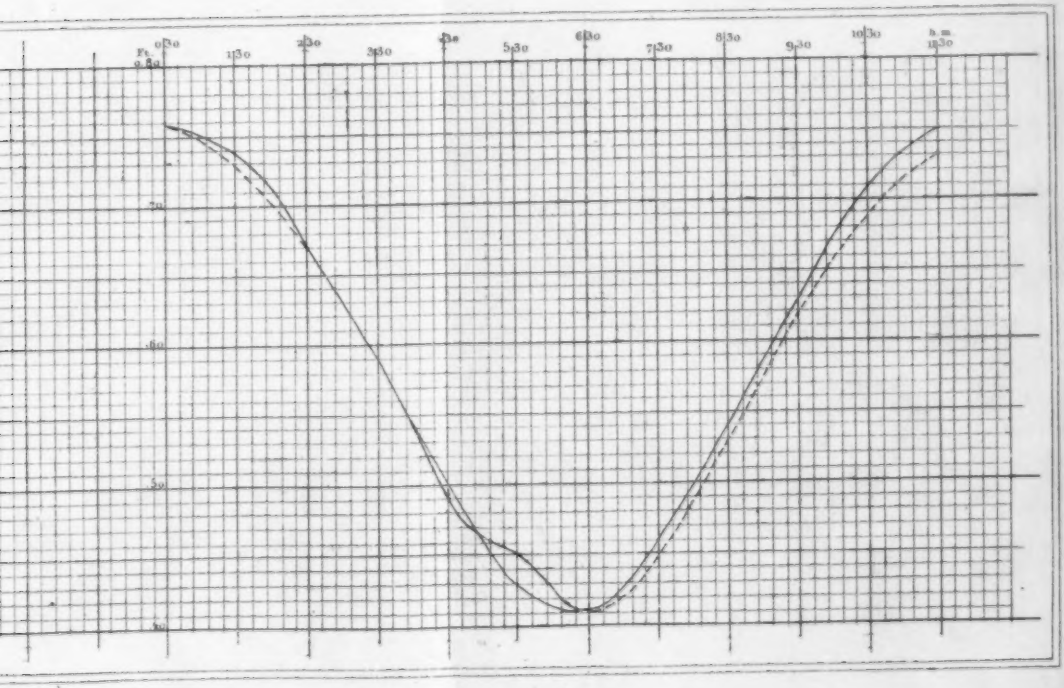
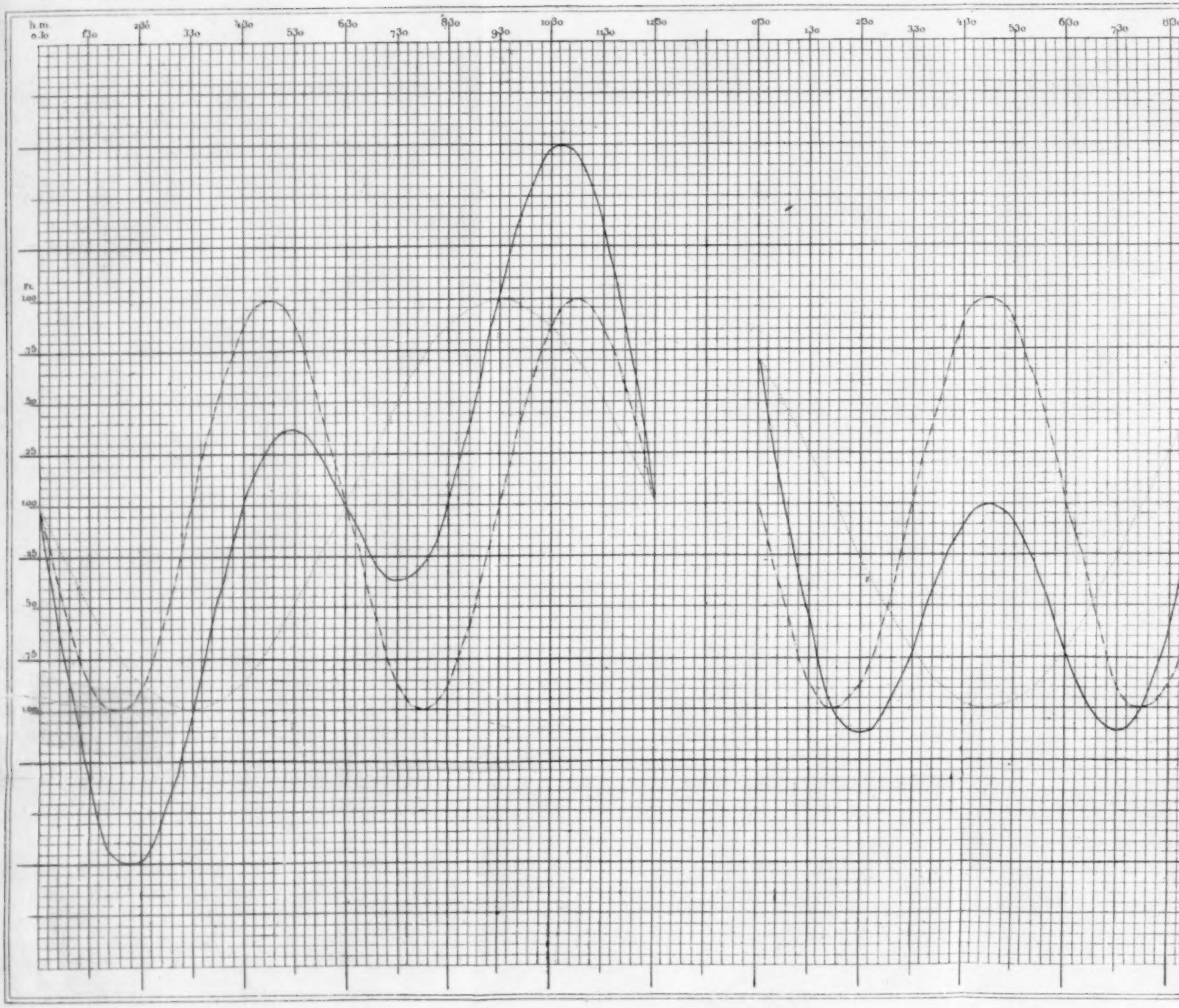


Plate No. 5

Showing change of compound curve with change of E.

No. 1 E = 9 hours, S = D

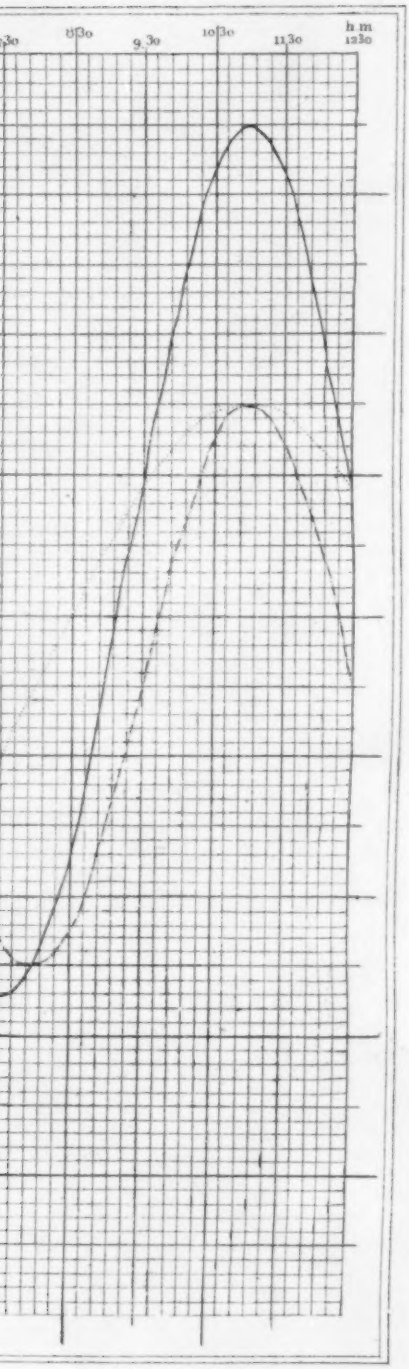


——————— *Semi-Diurnal Curve (S)*

——————— *Diurnal Curve (D)*

——————— *Compound Curve*

No. 2 E=12 hours, S=D

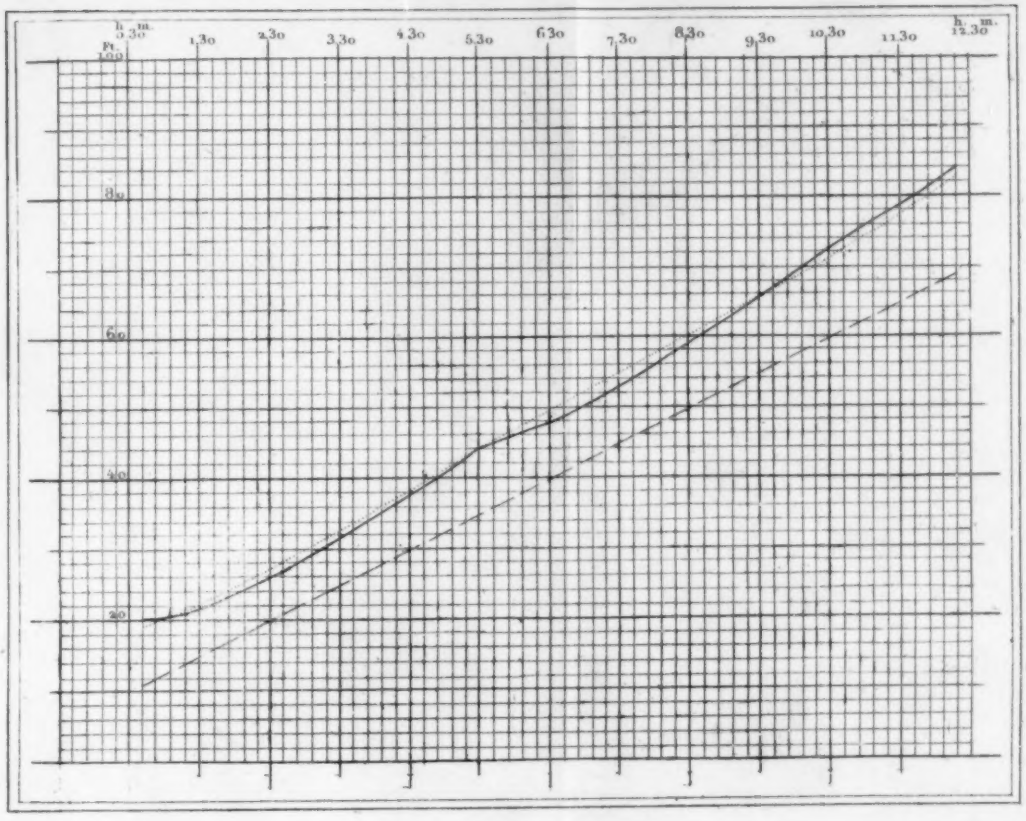


No 2

Plate No. 3

Whole year 1851-2

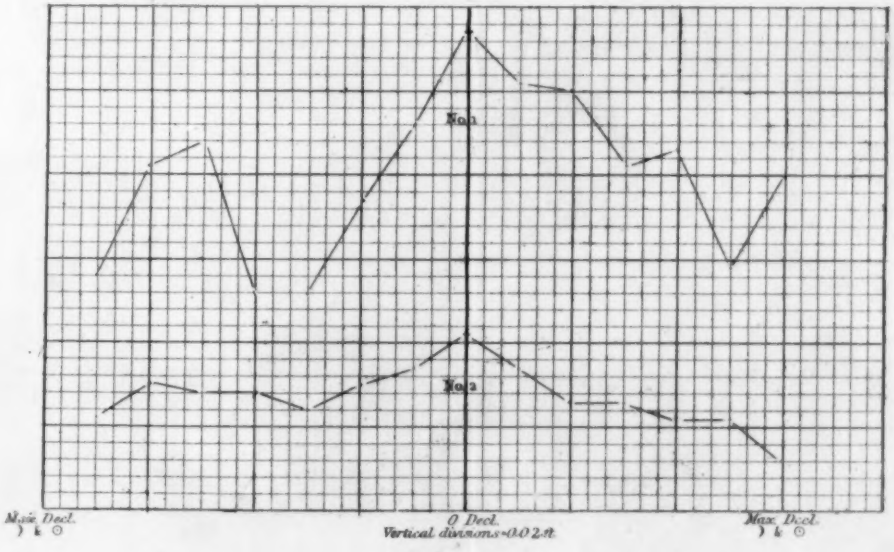
Comparison of Diurnal Inequality from observation and discussion of Curves with Moon's Decl



..... Observation

_____ Discussion

— — — — Sine of Twice Moon's Decl.

Plate No. 6

Showing the mean level of the water at Key West the observations arranged according to the declination of the Moon (No 1) & the Sun (No 2) respectively